







The Milbank Memorial Fund  
**QUARTERLY**

CONTENTS

	<i>Page</i>
IN THIS ISSUE	381
SOCIAL CHANGE AND MENTAL HEALTH <i>H. B. M. Murphy, M.D.</i>	385
CULTURES AS CAUSATIVE OF MENTAL DISORDER <i>Alexander H. Leighton, M.D. and Jane M. Hughes, Ph.D.</i>	446
THE INFLUENCE OF WAR AND POSTWAR CONDITIONS ON THE TEETH OF NORWEGIAN SCHOOL CHILDREN. IV. CARIES IN SPECIFIC SURFACES OF THE PERMANENT TEETH <i>Guttorm Toverud, Louis Rubal and Dorothy G. Wiehl</i>	489
AGE HEAPING IN THE UNITED STATES CENSUS: 1880-1950 <i>Melvin Zelnik</i>	540

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*In Memorium*

*The Milbank Memorial Fund regrets to report the death on April 20, 1961 of Enid Mildred Shultes who was in charge of the production of the Fund's publications. Miss Shultes joined the staff of the Fund in 1925 as editorial assistant and was placed in charge of the production of all of the Fund's publications in 1934, a post she held until the day of her death. During her tenure, she was involved in the development of the Fund's Quarterly from a small house organ and news bulletin, to a scientific journal known in public health and demographic circles around the world. With her interest and skills, she helped to establish and maintain the high standards which characterize the Fund's publications. The Officers and Staff of the Fund deeply regret the passing of this longtime friend and associate.*

## IN THIS ISSUE

THE great and growing interest in mental disorders, especially in their prevention, has stimulated much epidemiological research designed to obtain evidence on the causes of different kinds of mental disorders which can be the basis for developing preventive programs. Aware of this interest and of the many recent studies which have evaluated data on associations between a variety of factors and mental disorder, in 1959 the Milbank Memorial Fund sponsored a Round Table meeting at Arden House, at which present knowledge about causation of mental disorder was discussed. In preparation for this meeting, eight distinguished authorities were asked to prepare review articles summarizing the evidence relating to different kinds of causes which had been thought to lead to mental disorders. The papers were distributed to the participants in advance of the meeting. At the meeting, the discussion of each review paper was opened by a previously designated participant; a general discussion followed; and the reviewer then added his own comments on the discussion.

The Fund's formal publication of the Proceedings began in the January 1961 issue of the *Quarterly* (Vol. xxxix, No. 1), and is completed in this issue. Included in these Proceedings are the review papers, the opening discussions, summaries of the subsequent general discussion, and the closing remarks of the authors of the review papers. The first paper of the meeting—"Genetical Etiology in Mental Illness" by Professor Jan A. Böök—was printed separately from the Proceedings series in the July 1960 issue of the *Quarterly* (xxxviii, No. 3), so that, unfortunately, the discussion of the paper was not published at that time. However, this material will be added to the Proceedings which will be collected in a volume entitled: **CAUSES OF MENTAL DISORDERS: A REVIEW OF EPIDEMIOLOGICAL KNOWLEDGE, 1959**, sometime in the latter part of 1961.

The reader is referred to the January 1961 issue of the *Quarterly* for a brief introductory statement outlining the objectives of the meeting and for the abstracts of all of the review articles presented at the meeting.

Two of the review papers, together with their discussion, will be found in this issue:

Social Change and Mental Health

H. B. M. Murphy, M.D.

Since the Enlightenment it has been repeatedly claimed that rapid social change was productive of mental disorders. The evidence advanced in support of this thesis is separated into two concepts: 1. Change as a specific factor in the production of certain cases of mental disorder. 2. Change that is disturbing to everyone but which produces a clinical form of the illness only in presently or potentially sick persons. This extensive review of the very large literature on migrating populations and populations undergoing social change now permits one to ask many different and more precise questions. Dr. Murphy concludes that non-Western peoples undergoing Westernization show an increase in identified psychopathology. However, whether this is due to an increase in prevalence or to improved facilities or other factors is not clear. The problems of investigating these questions are dealt with at some length.

Cultures as Causative of Mental Disorder

Alexander H. Leighton, M.D.

Eleven different ways in which culture is thought to increase the frequency of particular mental disorder is examined and a review of the evidence for each type of linkage is examined separately. This classification is expounded together with examples of studies, no one definitive, which are directed at testing each theory. The fact that a global, cross-cultural classification of mental disorders does not exist is discussed as a gap in the technical resources for studying these issues.

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The decrease in dental caries for Norwegian school children during the War years and the increase in the post-war period

have been described by Professor Guttorm Toverud, in three articles on "The Influence of War and Postwar Conditions on the Teeth of Norwegian School Children" which were published in the *Quarterly*. In this issue, a fourth article on "Caries in Specific Surfaces of the Permanent Teeth" by Guttorm Toverud, Louis Rubal and Dorothy G. Wiehl extends the analysis to a consideration of changes in caries activity in each surface of the permanent teeth. Patterns of accumulating caries vary widely for the different teeth and, also, for different surfaces in the same tooth. These patterns are described and the differences in effects of changes in oral conditions on the various surfaces are discussed. In young teeth, that is, in teeth at an early post-eruptive age, caries activity was affected very soon after new conditions became operative. In older teeth, such as incisors and first molars of 12 and 13 year-old children, the previous caries accumulation in the most susceptible surfaces kept the caries prevalence in these surfaces at a high level for several years after war conditions had affected other surfaces. In first molars of these older children, caries rates for occlusal surfaces showed no reduction until the post-war years, but sharp reductions occurred in the mesial and distal surfaces in the second or third year of the War.

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A paper presented in this issue "Age Heaping in the United States Census: 1880-1950" is a description of the techniques used and the results obtained by Dr. Melvin Zelnik in his effort to diagnose and apply corrections to the tendency for people to report their ages to census enumerators in round numbers or in certain even numbers. This task was carried out as a means toward another objective—that of securing reasonably correct single-year age distributions in past censuses in order to estimate the annual births and birth rates in the period before the birth registration areas included all the states. The larger job is the subject of a recently completed doctoral dissertation by the author; the present job represents a revised chapter in that thesis.

## PARTICIPANTS

### IN ROUND TABLE ON CAUSES OF MENTAL DISORDERS: A REVIEW OF EPIDEMIOLOGICAL KNOWLEDGE, 1959

JAN A. BÖÖK, M.D., *Professor and Director, The State Institute for Medical Genetics, University of Uppsala, Sweden*

FRANK G. BOUDREAU, M.D., *President, Milbank Memorial Fund*

G. MORRIS CARSTAIRS, M.D., *Director, Medical Research Council Unit for Research on the Epidemiology of Psychiatric Illness, London*

JOHN H. CUMMING, M.D., *Director, Mental Health Research Unit, New York State Department of Mental Hygiene, Syracuse*

PAUL M. DENSEN, D.Sc., *Deputy Commissioner of Health, The City of New York*

H. WARREN DUNHAM, Ph.D., *Professor of Sociology and Anthropology, Wayne University; and, Director, Epidemiological Studies, Lafayette Clinic, Detroit*

ERNEST M. GRUENBERG, M.D., *Milbank Memorial Fund*

E. GARTLY JACO, Ph.D., *Director of Socio-Environmental Research, Cleveland Psychiatric Institute and Hospital; and, Associate Professor, Departments of Sociology and Psychiatry, Western Reserve University*

GEORGE JAMES, M.D., *First Deputy Commissioner of Health, The City of New York*

MORTON KRAMER, D.Sc., *Chief, Biometrics Branch, National Institute of Mental Health, Bethesda*

ALEXANDER H. LEIGHTON, M.D., *Professor of Sociology and Anthropology, Cornell University; and, Professor of Psychiatry (Soc. Psych.), Cornell University Medical College*

PAUL V. LEMKAU, M.D., *Professor of Public Health Administration, The Johns Hopkins University School of Hygiene and Public Health*

BRIAN MACMAHON, M.D., *Professor and Head, Department of Epidemiology, Harvard University School of Public Health*

H. B. M. MURPHY, M.D., *Section of Transcultural Psychiatry, Allan Memorial Institute, McGill University, Montreal*

BENJAMIN PASAMANICK, M.D., *Professor of Psychiatry, Ohio State University College of Medicine; and, Director, Research Division, Ohio State Department of Mental Hygiene and Correction, Columbus*

DONALD D. REID, M.D., *Professor of Epidemiology, London School of Hygiene and Tropical Medicine*

GEORGE ROSEN, M.D., *Professor of Health Education, Columbia University School of Public Health and Administrative Medicine*

## STAFF

MISS DOROTHY G. WIEHL

MATTHEW HUXLEY

RICHARD V. KASIUŠ

## SOCIAL CHANGE AND MENTAL HEALTH

H. B. M. MURPHY, M.D.

### INTRODUCTION

THE belief that social change has an adverse effect on mental health has a respectable ancestry. Esquirol thought that his figures showed that "Les perturbations sociales de cette époque [1830-31] ont exercé leur influence sur la production de la folie, nonseulement par la frayeur et par l'exaltation politique mais par le bouleversement dans la position sociale de beaucoup des individus." [*sic.*](25) Maudsley forty years later, expressed much the same idea when he said, *a propos* the reported rarity of mental disorder in primitive peoples, "[The savage] is extraordinarily conservative . . . he is free therefore from the perils which to unstable natures lie in the excitement produced by revolutionary change and the adjustment to the new relations exacted thereby." (57) The focus today has shifted from political revolution to other forms of change, but the statement in Ruesch's well-known monograph that "The difficulties encountered in the process of acculturation are largely reflected in the statistics of mental disease" (72) is directly in line with these earlier thinkers.

When we come to examine references to this belief, however, we find two different strains of thought which have very different meanings for preventive mental health work. On the one hand we have simple claims or simple demonstrations that change has been a factor in the development or precipitation of mental disorder in specific cases, and no inference is made regarding the effect that the same change may have on other individuals or groups. Such reports invite research into the particular combinations of factors, personal and social, which create a mental health hazard and leave the question open as to whether prevention of the effect might be achieved through some factor other than the change itself. On the other hand there are studies, both empirical and theoretical, which conclude with the inference that social change has the same type



of effect on most or all people, with the clinical cases being the visible part of the iceberg. Such a viewpoint is implied in remarks about immigrants wearing out earlier than other people, in certain theories of social disorganization, and in statements of the type of Wolff's: "In a rapidly changing society the anxiety-inducing factors are carried along in the traditions of society and outlive the anxiety-resolving factors." (90) According to this view, therefore, it is not particular combinations of factors which create the danger but change itself; and since social change is something which we cannot stop and which, at least in common opinion, is increasing in tempo, it follows that there is virtually nothing that can be done to prevent the effect. Obviously which theory of social change is correct has importance for mental health planning. If social change has a generally adverse effect, then the likelihood of increasing mental disorder in areas of rapid change must be faced, treatment services will have to be stepped up, and research into other factors in mental health must allow for this effect. On the other hand, if social change in itself does not have an adverse effect but is dangerous only when in combination with certain other factors, then it follows that belief in change as the main factor is going to prevent us from attacking other factors where preventive action might have considerable success.

In an attempt to resolve this dilemma the present paper will review what evidence exists regarding the effect of social change on mental health in general, and then examine what concomitant factors may be sharing in the result. If what may be called the *general hazard* theory is correct, then, for every situation in which such groups can be compared, we should expect to see higher rates of mental disorder in people who have undergone a given type of social change than in people who have not undergone it, other factors being allowed for. If, after due allowance for such other factors, there are sufficient instances where social change does not seem to have had any adverse effect, then it seems reasonable to conclude that the general hazard theory has been found wanting and that the other is

the more probable (though, of course, still other theories are possible, and might prove to be still more applicable).

The main types of change which will be reviewed are migration from one country to another, migration from one region to another within the same country, the changes from war to peace and vice versa, and the adjustment of non-Western peoples to modern Western civilization. Many more examples could be chosen, and it would by no means be agreed by all that the ones so chosen represent true social change. These cases do, however, constitute the main types of general change about which we have epidemiological data, and they are among the ones which have been most commonly cited as examples of social change in the past. Whether particular instances should have been included will be discussed as the need arises; for to stop here and consider the nature of social change and the various definitions which have been suggested for it in the past, would mean that my given task—that of reviewing the epidemiological literature—would never be reached at all. There are serious problems involved in whatever concept of social change one might choose to use, especially when, as in the present case, its relationship to individuals is important; but these cannot be dealt with here. The reader is asked to assume that, unless there are strong reasons to the contrary, all instances of change cited here *are* instances of some form of social change, though not necessarily of social change as limited by any one popular definition.

Since mental hospitalization is that index of mental disorder which is most commonly cited and most easily ascertained, most of this paper will necessarily concern itself with such hospitalization data, and these will be treated first. However, it is recognized that in many ways this index is an unsatisfactory one. In consequence, other indices of mental disorder will also be considered, together, in a subsequent section.

#### ADMISSIONS TO MENTAL HOSPITALS

*Immigration.* At one time the striking relationship between

immigrant status and mental hospitalization seemed, at least to some, clear evidence of the malignant effects of one form of social change. Today the relationship has become quite doubtful, and its meaning equally so.

This change has come about through the analysis of related variables. Before the 1930's, papers were regularly published indicating that the immigrant sections of the United States and Canadian populations had proportionately two to three times more patients in mental hospitals than the native-born sections.<sup>1</sup> Then the work of Ødegaard (63) and Malzberg (57) demonstrated that when variations in age and sex were controlled, most of this difference could be accounted for and that what differences remained applied to virtually all ethnic or cultural groups, to both sexes, and at most ages. In consequence, although the differential was less, its association with the *migration* factor itself seemed strengthened for the racial explanation now seemed impossible, and that factor had previously been the main competitor. However, what was temporarily forgotten was that if differences in age and sex distribution between the native and immigrant sections of the population could account for the bulk of the difference in rates, other differences between these two groups of the population might be able to account for the rest. It had long been known that immigrants differed from natives with respect to average social class, predominant type of residential milieu, years of schooling, and ratio of single to married; and it was also being learned at this time that associations existed between such factors and mental hospitalization rates (77, 26, 27). In consequence, the possibility existed that if these factors were also controlled for, the immigrant/native difference in mental hospitalization rates would disappear completely.

Such a disappearance has not yet been convincingly demonstrated, but two papers have pointed in the expected direction. The first is a relatively recent one by Malzberg, using the 1940 instead of the 1930 Census. (The 1930 Census gave an un-

<sup>1</sup> See reviews in Ødegaard (63) and Gillon (31).

avoidable bias to his earlier studies in that they had to be related to the Depression, which was liable to hit the immigrant harder than the native.) In this paper (54) he shows that when adjustment for broad rural/urban differences in residence is added to the traditional adjustment for age, standardized mental hospitalization rates for immigrants and natives in New York State become virtually the same (Table 1).

Were we looking only at the mental hospitalization rate and not at the mental disorders for which it is the indicator here, this finding would be convincing, but one cannot help observing (Table 1) that while admission rates generally have become about equal, admission rates for the major psychoses still show a significant differential, with the immigrants showing an excess in both sexes. One very possible interpretation of these findings might be that the relatively recent trend towards seeking hospitalization for the minor disorders (such as the neuroses) had moved much faster among the native-born than among the immigrants. Hence, while this paper is suggestive, it cannot be said to demonstrate convincingly that immigrant mental health is the same as native mental health, once crude urban/rural differences in residence are allowed for.

The second paper is Clark's relatively neglected study of schizophrenia analyzed by nativity and occupational status

Table 1. Ratios of foreign-born to native-born rates of mental hospitalization, in New York State hospitals, first admissions only, 1939-41; standardized for age and urban/rural distribution by sex and main diagnoses.

	MALES	FEMALES	TOTAL
1. All First Admissions	95.9	109.1	102.0
2. Dementia Precox	128.8	124.2	127.1
3. Manic Depressive Psychoses	93.7	107.9	103.4
4. General Paresis	108.4	79.4	101.4
5. Alcoholic Psychoses	61.3	90.6	62.3
6. Arteriosclerotic Psychoses	103.5	117.8	110.1
7. Involuntional Psychoses	84.9	128.6	113.4
8. Senile Psychoses	97.5	129.7	115.8

Items 1-4 standardized for population of New York State aged 15+ on April 1, 1940; item 5 on population aged 20+ at same date; items 6, 8 on population aged 45+ and item 7 on population aged 35+; all in intervals of five years.

Source: Malzberg (54).

(16). The broad finding of the paper is that immigrant rates are, on the average, higher than native ones after occupational status is controlled for (at least, this is one of the general conclusions that can be drawn; the paper really focuses on the occupational groups, nativity being an incidental). However, what is interesting at the present juncture is that there were some quite major occupational categories in which the rates for immigrants were either the same or lower than the rates for native-born. Of his 17 categories there were three in which the foreign-born had lower rates than the native-born, three (including the numerically important office worker and domestic servant groups) in which the rates were virtually the same, and four others in which the excess in the immigrant rates was less than what would usually be accepted as significant. Hence although the immigrants had a higher rate on the average, there were quite important sections of the population where this did not appear.

In neither of these studies were education, marital status, or detailed location of residence (e.g. slum or suburb) allowed for, and we have some reason to expect that these would have had an influence on the resultant rates. And another unconsidered factor, which we will be discussing shortly, was internal migration. Consequently, it cannot be said that we know where we stand much better than before. All that we can say is that as more concomitant factors are allowed for, the gap between immigrant and native rates decrease, and that Clark's study—with some groups showing a difference in rates and others showing none—seems better explained in terms of the multiple factor theory than in terms of the general hazard one. However, it must be remembered that his study was only of hospitalized schizophrenia, and hence of limited significance.

These studies have all dealt with the Western hemisphere. When we come to look at studies done elsewhere there is, in general, less detail to be found and the results are equally confusing. In Britain, the incidence of mental hospitalization in immigrant displaced persons has been shown to be extremely

high (60), age being eliminated as a disturbing variable; and in France, the North African immigrant group probably have a high incidence as well (18).<sup>2</sup> In both these instances, however, there is clearly more than just social change acting as a stress. The D. P. had lost his country and had been through exceptional strain during the war; the African had very low socio-economic status and was faced with a conflict of interests or conflict of loyalties since in his homeland a nationalist struggle against the French was being waged. We cannot, therefore, simply take these groups as people exposed to cultural change and to no more specific stress; the additional stresses which we would like to take into consideration are not ones we can properly allow for.

From Israel (78) and Singapore (61) come contrary pictures, i. e. reports of immigrants having less mental hospitalization than natives. The Israeli data are crude—not standardized for age or anything else—and in the report from which they are drawn one gets the impression that the country's mental hospitalization patterns show some peculiar characteristics. Nevertheless, the figures do indicate that both the European and the African/Asian immigrant groups have lower rates of mental hospitalization than the native-born (78) despite the fact that Israel imposed no real medical limitations on entry, that the social adjustments there would seem to be unusually demanding on immigrants, and that the age structure of the various groups would be in favor of the local-born having the lowest rates. Two counter-balancing points that have to be remembered here are that the immigrant's mental disturbance may be taking a sociopathic instead of a psychopathic direction (crime figures suggest this), and that the local-born are really only a little more established in a cultural tradition than the newcomers.

Nevertheless, the figures as they stand pose yet a further

<sup>2</sup> Eitlinger's admirable study of refugees in Norway, which has just reached me, also shows refugee immigrants to have very high rates of mental hospitalization, but the same remark applies here as to the D.P.'s in Britain. *Psykiatriske Undersøkelser Blant Flyktninge I Norge*, Oslo, 1957.



challenge to the general hazard theory of social change, and this is also true of the Singapore material. In this last (unpublished) study, population estimates rather than census figures had to be used, which naturally limits the degree to which conclusions can be drawn. Nevertheless, the ratio of immigrants to native-born in mental hospital first admissions was considerably lower than the ratio which has been projected from census and other sources, and when the probable distribution at different age groups within the population was taken into consideration, native rates exceeded immigrant ones at most ages (61).

The lack of unanimity about these findings favors the combination-of-factors theory much more than the general hazard one, but it does not mean that the latter can be ruled out. There are at least two reasons for believing that these findings, even when more thoroughly worked out, would tend to be biased. In the first place it has been argued, with some justification, that mental hospitals tend to be used more freely by those familiar with a country's customs than by immigrant foreigners and that, in consequence, immigrant patients will tend to remain out of hospitals longer than natives. In the second place, immigrants during this century have had to face increasingly strict pre-migration examinations and interviews which have, as a partial intention, the elimination of those most liable to mental breakdown. One may doubt the efficiency of such screenings, but they probably do have an effect, not only during consular interviews but also on the process of self-selection, so that persons having a disturbed family history refrain from seeking emigration lest this history should have to be revealed. Hence it could be argued that immigrants today constitute, not an adversely self-selected group as Ødegaard and others before him have suggested, but a favorably self-selected group, healthier than the average. If this were so—and Dayton's figures showing a relative decline in the crude immigrant/native-born rate differential between 1917 and 1933 could be interpreted as evidence in favor of this argument



(19)—then the possibility exists that the immigrants' mental health could have been worsened by social change without the rate of hospitalization (or rate of any other sort of disorder) rising above the average for the surrounding population. Since neither of these factors should be operative with internal migrants, i.e. those who move within their native country, one possible way to seek better evidence is to turn to this group.

*Internal Migration.* Evidence of possible relationship between internal migration and mental disorder does exist, but it is as well to say straightway that it is similarly ambiguous in its conclusions. New and equally grave problems present themselves in the place of those which may have been solved by switching from external to internal migration. The first problem is one for the theoreticians, namely, whether migration within a country—and more especially between such parts of a country as may be expected to have similar attitudes towards mental hospitalization and related matters—constitutes a true form of social change. This question can be left aside for the moment, but obviously any evidence that internal migration is no hazard to mental health becomes irrelevant if internal migration is not accepted as a valid example of social change. The next problem is that of self-selection. While internal migration does not necessitate medical examinations and interviews such as may, by threat or actual operation, affect the nature of an immigrant population, the former is exposed to different biasing factors. In the United States, for instance, there is a tradition that the more able rural youth moves to the town, but against this stand some recent reports of Robins and O'Neal. These show that former patients of an urban child guidance clinic demonstrated, thirty years later, both much more mental disturbance than a control group and significantly more geographic mobility, 29 per cent having left their city as compared with 15 per cent of the controls (71). To gauge the resultant balance of such selective forces on the mental health of both a general migrant population and special sections of

that population would need an elaborate field enquiry and might not be possible even then. It is thus worth remembering that the following conflicting findings are drawn from different types of social and cultural settings in which different traditions or patterns of migration may prevail, resulting in different selection biases.

The best known study on the subject is that of Malzberg and Lee, and refers to New York State (55). They showed that, of the current population, those who were resident outside the state five years previously (this being the census criterion) had a very much higher rate of mental hospitalization than those who were resident within the state at the same date, this difference applying to all ages, both sexes, urban and rural dwellers, and to both whites and Negroes. Occupation and nativity were not controlled for, and, as the authors themselves noted, the question of undeclared previous admissions to mental hospital in some other state might bias the result in favor of those classed as non-migrant. An undue proportion of the admissions seemed to have been in New York State less than a year, and although this could be interpreted as reflecting the stress of recent change, other interpretations seem more likely. A study from the Paris region gives similar general results, but without the same allowance for age, type of residence, etc. (82).

Against these (dealing now only with hospitalization studies) must be put the findings of Ødegaard in Norway and an incidental finding in Jaco's social isolation study. Jaco (41), contrasting districts in the same city with high and with low rates of schizophrenia, found that the high-rate area had not more geographic mobility than the low-rate one, but less. His results thus do not refer to total mental hospitalization, and since social class was rather an obvious differentiating variable between his two types of area, the fact that this was not controlled for weakens the significance of the observation for present purposes. It is the Norwegian study which carries the greatest counterweight. There, Ødegaard (64) showed that

while migrants in Oslo had slightly higher rates than those born and still resident there at time of census or hospitalization, in all other districts of Norway the reverse was the case, the balance for the whole country being definitely in favor of the migrants as the healthier group. Some question must exist regarding these findings since, although 'standardized' rates are used, the type of information on which customary methods of age standardization are based, i.e. the ages of the population-at-risk, does not seem to have been available.<sup>3</sup> But I do not believe an error here could annul the general finding.

As it stands, the evidence is mixed and inconclusive with the weight rather in favor of migrants having raised rates of mental hospitalization, but with no indication whether this would derive from self-selection or from the social change which is presumed to be experienced. One new point that does come out, however, is the difference between Oslo and the rest of Norway as regards migrant/nonmigrant rate differentials. Oslo, Paris and New York are all main cities of their countries. Ødegaard's finding, especially when joined by those from Baltimore and Texas which will be considered below, suggests that the migration to a metropolitan area or main city of a country may have a different significance for mental health from migration elsewhere.

It may be that life in such cities has a different character from that in other cities, or that a different type of person is attracted there, or it may be that patients are drawn there by reports of better psychiatric care. When the Malzberg and Lee book first reached me I was studying diagnostic criteria in the psychiatric department of a New York City municipal hospital and sought to check the last possibility. Of the few patients investigated, three proved to have moved into New York City only a few weeks previously and to have had either premonitions of illness or actual mental hospitalization down South shortly before. In these patients it is my opinion that

<sup>3</sup> There is some discussion of this in Dorothy Thomas' introduction to Malzberg and Lee, *MIGRATION AND MENTAL DISEASE* (55).

the move to New York to live with a sister, etc., was largely motivated by the desire to get good care.

*War and Peace.* The easiest method of avoiding interpretative problems induced by self-selection is to choose changes in which the individual has had little or no say. Two types of social change in which this may be said to apply, and regarding which some data exist, are the onset or cessation of a war, and the invasion of a non-Western people by a Western culture. In both instances there are, as always, further factors which can confuse the issue, but the subjects deserve looking into.

The onset and cessation of a war mark a major social change for a minority of individuals and a more doubtful change for the majority. Those who join the armed forces for the first time then, or who get interned, or who become displaced, or become refugees in a neighboring country, undoubtedly find marked changes in the society with which they are in everyday contact. For the rest of the people, even though there may develop new social attitudes, new institutions (food rationing, for instance), and new patterns of face-to-face relationship (the sharing of bomb shelters), the main social patterns and structure around them remains the same, and hence some might say that essentially no change was occurring. However, psychiatrists at such times have recorded numerous *individual* cases of people breaking down because of the events in the wider society around them even when they were not soldiers, internees, etc., and it is thus appropriate to ask what the statistical effect was on the people at large.

The result is unusually unanimous. In the previous century Esquirol and his followers were noting that the incidence of alienation nearly always dropped with war. In the present century we have data regarding World War II, for France (1A, 1B), Belgium (20), Britain (38, 30), Denmark (79), and Norway (65), all showing a clear decline in mental hospitalizations in the first years, and only Denmark showing any later rise. (In Britain, France and Belgium, of course, developments

in the later years of the war were obscured by the destruction and diversion of mental hospitals.) The Danish rebound seems quite likely to have been associated with the peculiar problems of loyalty which the Danes faced in these years, and in any case did not seem to raise the incidence rate above the mean long-term trend line (see Svendsen's Fig. I (79)). In Norway, where questions of loyalty were more clear cut, Ødegaard (65) found no later rise and no evidence that the drop had derived from an increase in the interval between onset and hospital admission. In most of the countries cited there were some extraneous factors which might have helped reduce the incidence rate, but all the writers reporting on the subject seem to agree that these were insufficient to account for the size of the drop which was found, and that a genuine drop in psychosis incidence probably did occur. What happens at the change from war to peace, however, is less clear. Denmark and Britain during 1945/46 saw definite rises in mental hospitalization above the average of the wartime years and possibly above what one can guess to be the long-term trend line. In Denmark the rise was of brief duration (79) and clinical data suggest that the fact of change did have some relevance (though here again one must note that for the Danes there was the conflict of loyalties problem which was not an essential part of usual war to peace changes (70)); in Britain the rise was the prelude to the much greater rise of the succeeding years (30), a rise associated with a change in popular attitudes towards psychiatric care and with the introduction of the National Health Service. In Norway, however, Ødegaard's averaged figures show no clear rise above the long-term trend which pre-war years projected (65), and in Singapore there was a long delay before pre-war levels of hospitalization were reached, even though the supply of psychiatric beds was outrunning the demand and though other forms of hospital use (non-psychiatric) very quickly overtook pre-war levels (61).

The upholder of the general hazard theory can make three main points in answer to these findings. He can say that the

change from peace to war and from war to peace is not a social change in the true meaning of the latter term; it is a change in the society's situation, but not in the society itself. Next, he can say that hospital data, especially at such times, are not valid indicators. Finally, he can say that even if hospital data were valid for this purpose, the correct hospital data have not yet been examined, for the adverse effect may be a delayed one showing itself perhaps only in a much later increase in the degenerative disorders of old age.

To these objections there are no good answers. Indicators of mental disturbance other than hospitalization are virtually not available for this type of change, and studies of the delayed effect of war in general are not only lacking but would be exceedingly difficult to carry out. On analogy with immigration a delayed effect seems plausible, since immigrants do show an excess of mental disorder in old age (Table 1); but, again on analogy with immigration, there is the problem of deciding whether any such hypothesized excess would outweigh the drop in hospitalizations which had occurred earlier. In any case, to know what particular events in a population's earlier history should be incriminated in a rise of psychosis in old age is very difficult. Only on the question of whether the changes from war to peace and from peace to war are social changes or not is there something more to be said. For here one can turn to special sections of the population—military personnel, internees, refugees, etc.—for whom the change was much more striking.

The difficulty with the latter groups is that their experiences during the period of war are what most observers, and most of the subjects, would consider as stressful; and if we find any rise in mental hospitalizations (or in other indices of mental disorder) then it has to be decided whether this rise was mainly associated with the social change, or with the social situation which resulted from that change. Moreover, in the case of the armed forces there is the question whether this was a group successfully selected for superior health—in which case their



average rates of breakdown should be lower than those for other populations, if other factors were equal—or whether the selection procedures were largely irrelevant.

With the armed forces it seems best to confine attention to the psychoses, since these are the conditions which would almost certainly have required mental hospitalization in peacetime, whereas of the other types of conditions many were acute battle exhaustions; many others were referred to the psychiatric services as a way of getting them out of a unit where they were not accepted; and some of even the seriously neurotic would not have received hospitalization under civilian conditions. In the United States Army there were, according to Appel (3), 45 neuropsychiatric admissions per 1,000 per annum, of which about 7 per cent were psychotic, thus giving a psychosis rate of about 310 per 100,000 per annum. This is at least double the New York State civilian rate for males 20-39. Moreover, a relatively high proportion of the psychotic breakdowns appeared to have occurred in the early period after induction. Seventy per cent of a sample of psychotic soldiers discharged before proceeding overseas and studied at Bellevue, had broken down in their first 5 months (40), and 50 per cent of Paster's home-base cases broke down in their first year (67). No incidence rate can be calculated for these early breakdowns, but it seems almost certain that it was much higher than would have occurred in the same population if it had not been inducted, and that it was the social change rather than the conditions met with which was the major environmental factor. Seventy-seven per cent of a sample of early psychotic breakdowns studied by Will (87) had never previously been away from home, as compared with 35 per cent of controls, and Klow has pointed out (48) that the type of disturbance found in such patients, though usually called schizophrenia, was more an acute, confusional, often paranoid state showing rapid and complete recovery. (Other reports dispute this last finding, but the difference may relate to the speed with which treatment was instituted.) However, these United States results



are not necessarily typical. In the Indian Army in peacetime, the incidence of psychiatric admissions of all types was only 1.06 per 1,000 per annum for Indian troops and 2.80 for British troops. The highest psychiatric admission rates ever reached during World War II (on the Arakan front, where conditions were unusually anxiety-provoking) by these two groups was 6.6 and 12.9 per 1,000 (6, 88).<sup>4</sup> These are very different figures from the 45 per 1,000 which Appel reports for the United States Army (3). While, therefore, induction into the army would, from United States data, appear to be associated with a raised incidence of hospitalized psychosis despite efforts to screen out the pre-psychotic, data on troops from other cultures do not necessarily confirm this finding. In regard to the Indian peacetime and early wartime figures, a possible source of error may lie in the fact that commanding officers could discharge sepoy from the force without stated reason within their first six months of training, and hence might have diverted certain cases from medical hands. Nevertheless it is possibly significant that no papers on acute psychotic breakdowns in soldiers during their early training period seem to have been published by British or other European workers.

Regarding the change from war to peace as it affects soldiers and ex-soldiers, there is virtually no evidence touching on mental hospitalization. Clinical papers exist and there is Curle's well-known study of readjustment in a broader sense (17), but no statistics exist on the incidence of major breakdowns in the demobilization period. On delayed effect (or lack of effect) something does exist, however. In 1953, Canada's Dominion Bureau of Statistics included in its tables of statistics on mental hospitalization, the numbers reported to be veterans, this having been a specific enquiry on their reporting card. Table 2 below shows the more interesting items. They can be compared with the statement by the Encyclopedia Canadiana that "forty per cent of the male population aged 18-45 spent some

<sup>4</sup> Estimated comparative *psychosis* incidence in the two groups during the Arakan campaign stand at 177 per 100,000 Indian troops p.a. and 63 per 100,000 British (61).

time in the armed forces before the war ended" (i.e. World War II, not the Korean War). On the face of it this table suggests that while veterans probably produced more than

Table 2. Percentage of veterans reported by certain diagnostic categories in first admissions to Canadian mental hospitals, 1952.

DIAGNOSTIC CATEGORY	PER CENT OF FIRST ADMISSIONS, MALES ONLY
Hysterical Reaction	37.5
Somatization Reaction	36.8
Anxiety Reaction	31.0
TOTAL PSYCHONEUROSES	23.5
Alcoholism	28.1
Psychopathic Personality	23.9
TOTAL PERSONALITY DISORDERS	25.2
Alcoholic Psychoses	16.9
Paranoia	16.1
Manic-depressive Psychoses	8.4
Schizophrenia	13.8
TOTAL PSYCHOSES	11.1
TOTAL FIRST ADMISSIONS	11.8

Source: Abstracted from Table 34 of the [Canadian] Dominion Bureau of Statistics, *Mental Institutions. Report for 1952*

sions) they were producing much less than their share of the more serious disorders, and of mental hospitalizations generally.

The final type of social change to be considered here in connection with war is commitment to, and later release from, a P.O.W. or civilian internment camp. From the worst camps any information we may have about hospitalization and psychoses is probably of doubtful value, both because services were likely to be inadequate and because certain types of the mentally disturbed might be unlikely to survive long enough to be diagnosed and recorded. Yet from other camps we have in-

their share of mental hospitalizations for certain types of neuroses, they produced, in 1952, less than their share of the functional psychoses. In 1952 the mean age of Canadian veterans was probably about 32, which is also a common mean age for onset of schizophrenia. If, therefore, the reporting hospitals completed this item conscientiously, then it would appear that although veterans were producing more than their share of hospitalized minor mental disorders (first admis-

formation which is fairly reliable and which suggests that major mental disorders, at least during camp life, were less than normal. Thus, from the P.O.W. camps in the Singapore area during World War II the incidence rate for psychoses works out at approximately 40 per 100,000 per annum, plus a further 13 if one includes the reactive depressions (8). In the civilian internment camp in the same area the rate works out at 57 (69). For the P.O.W. camps for Germans in the United States, Gottschick (33) reports all forms of mental breakdown to be about the same as for the pre-war civilian German population, but schizophrenia to be less. In the displaced persons camps under this writer's administration from 1946 to 1949, the incidence of mental hospitalization works out at about 20 per 100,000 per annum (though minor mental disturbances were common) and correspondence with the Bundesgesundheitsamt suggests that for at least one other province (Land) similarly low rates were obtained. In the worst concentration camps the picture was different, but what has struck many trained survivors was the disappearance of certain forms of neurosis, the rarity of suicide (at least, in open form) and sometimes the rarity of the classic chronic psychoses. In Theresienstadt, admissions to the camp hospital with a psychiatric label were common, but these were sometimes subterfuges for getting someone extra care (49). On the whole it seems true that the incidence of major mental breakdowns in such situations was below average.

After release from such camps the picture may change, though data are regrettably scarce. The displaced persons who showed so little hospitalization while in their camps had a quite excessive rate on resettlement in Britain (60); one group of 2,600 Danish KZ survivors produced five suicides (if these be counted as psychosis equivalents) in four years (37); and a reactive depression was reported to be relatively common in those released from the Singapore civilian internment camp (69): that is the positive evidence. On the other hand, United States studies of repatriated P.O.W.'s, while reporting anxiety

and adjustment reactions, do not report any excess of major breakdown (10) and it is surprising that European countries (other than Denmark) have not reported any special problem in their ex-concentration camp people. Israel would seem to be one of the best sources from which to obtain a definitive answer, but satisfactory data there do not exist since their mental hospitals were for many years inadequate to their needs. In 1949, when the writer visited that country, psychotic reactions in resettling middle-European Jews (most of them from either ex-KZ or ex-D.P. camps) were reported to be high. In 1956, however, Sunier, as previously noted (78), found that although the immigrant from Europe had a much higher rate of hospitalization (patients *in* hospital; not admissions) than the Jewish immigrant from Asia or Africa, the local-born Jew had a higher rate still, both for mental hospitalization in general and for schizophrenia.

That is all that seems relevant to report on the relationship between mental hospitalization and the social changes accompanying the onset and cessation of a war. Once again there are conflicting findings, but on the whole the conclusion must be that the rate of mental hospitalization is not as a rule raised for groups who have experienced this type of change; rather it tends to be lowered. Before discussing the significance of this, however, it will be well to deal with the final instance of social change to be discussed here, and so have all the evidence relating such changes to mental hospitalization on the table.

*Acculturation to Western Civilization.* There are considerable difficulties in weighing mental hospitalization data referring to the Westernizing of non-Western peoples. It is rare that the availability and use of mental hospitals is the same in a (usually rural) primitive group and in a (usually urban) Westernized group from the same background; questions of what to regard as a major mental disorder are quite acute; and such matters as social class and education, which are differentially distributed between the two groups (if the con-

cepts, as commonly used, have relevance for such a comparison), are virtually never able to be allowed for. Nevertheless there are, from scattered sources, a number of instances where a Westernized or semi-Westernized group has been shown to possess unusually high rates of mental hospitalization or its equivalent, while a less Westernized section of the same people shows no apparent excess. If one accepts Moloney's report on the people of Okinawa (59) as indicating, at the very least, a less than average rate of psychosis, then the very high mental hospitalization rates which Okinawan immigrants to Hawaii show are striking (47). Again, the psychosis rate which Gans (29) uncovered among Javanese physicians who had all gone through Western schools and colleges, is strikingly higher than the rate among Javanese in the Netherlands East Indies army, being high for any population anywhere. In Singapore the English-educated Chinese male aged 20-49 had higher rates of mental hospitalization for nearly all categories of disorder than the Chinese-educated male (though not more than the illiterate) although the former was of higher average social class (61). Seligman has described classical types of psychosis occurring in association with Christian missions among the Papuans and Melanesians and has denied even hearing of such conditions in his many years of work among the same peoples in their original villages (75).

To weigh against these, I can find only doubtful evidence that the Westernized section of any non-Western people has proportionately less mental hospitalization, or its equivalent, than the less Westernized section, or even a rate that is no higher. Among Indian soldiers, the English-educated seem to have shown more neurosis but definitely less psychosis than the ordinary sepoy (88); but the question of rank and other military training also comes in here, as well as the impact of army life. In Tanganyika, according to Smartt (76), the proportion of Christian and Western-educated among psychiatric hospital admissions is not higher than among nonpsychiatric hospital admissions, but how accurately he was able to work

this out one does not know. Finally, in Hawaii the part-Hawaiian have significantly lower rates of mental hospitalization than the full Hawaiian (73), and while the latter cannot by any means be considered to be untouched by Westernization it could be argued that they are less Westernized than the part-Hawaiian. However, I doubt whether the distinction between part-Hawaiian and Hawaiian would be as carefully recorded by the mental hospital staff as it is at the census, and any tendency to record a patient as Hawaiian because he looks or speaks the language would markedly affect the apparent incidence rate for the full Hawaiian group, who are a small minority today.

It would thus seem that we now had at least some reasonably unanimous evidence incriminating social change, or at least incriminating one form of social change. For the balance of the evidence suggests that when non-Western peoples come in contact with Western civilization, and must to some extent adjust to it, mental hospitalization rates are higher than when the contact is slight and calls for little or no basic adjustment. However, there is another way of looking at such data, namely by comparing the rates of the transitional group, not with some non-Westernized people, but with a fully Westernized group, i.e. with Americans or Europeans themselves. Such a comparison seems equally legitimate and would seem to lead to a very different conclusion, namely that mental disorder, as measured by hospitalization, rises as one becomes more 'civilized'. This is an old idea, of course, and much argued against, but it is largely true that the same data that can be used to argue for the harmful effects of cultural change can equally be used to argue for the harmful effects of Western civilization. The part-Hawaiians may have a hospitalization rate which is lower than the full Hawaiians, but it is lower than that for the Caucasians also (73, 85). The detribalized African in Kenya may have a rate (13.3 per 100,000) which is higher than for the tribalized, but, as Carothers (14) remarks, this rate is still very low by Western standards or by the standards of the White in Africa,



although, "as it is unlikely that employed natives would not be certified if insane, this figure is probably a fairly true measure of the insanity in these people." In Singapore the rate for the English-educated section of the Chinese population is probably lower than for the Europeans there, and the rate for Eurasians (who are usually considered to be especially in a position of social instability and change) is definitely lower than that for the Europeans (61). In Cape Province, mental hospitalization for the Cape Coloured is lower than that for local whites, even when only urban sections of the population are compared (50). In the southern United States the rates for Hispano-Americans are lower than for Anglo-Americans, even though the latter carry the dominant culture to which the former are slowly having to shift (42). These are facts which make the sort of evidence cited in the previous paragraph much less clearly in favor of the general social change hypothesis. It is possible, of course, to argue that in the cases cited the transition group had reached a certain stability of cultural admixture, or that the Western group against which it was being compared was an unrepresentative one, selected or exposed to special stresses. It is also true that there is much more information which one would like to have in each instance, so that groups could be more closely compared. Nevertheless, it must be recognized that the evidence regarding cultural transitional groups, although much more unanimous than any other which has been considered here, is open to more than one interpretation and does not simply support the belief in a relationship between mental disorder and social change.

#### MENTAL HEALTH SURVEY DATA

Relatively few mental health surveys, using this term in its broadest sense, give data of relevance to the question of social change, and since such surveys are not numerous in any case there is little material to present here. If one were to use more impressionistic studies of self-selected populations, then there would be much more to report, but such studies concentrate



on the disturbed sections of their populations and usually leave out of consideration the possibility that just as much mental disturbance might be found in a population which has not had the experience under consideration. (This does not mean that the latter type of study is useless, but it means that its use is confined to determining what sorts of breakdown occur, not what their relative frequency is. And it is this last point which is being studied at the moment.)

Regarding immigration, the only mental health survey currently available is that of Weinberg in Israel, although the Midtown study should offer something in the near future. Weinberg, a psychiatrist, interviewed 280 immigrants from Holland to Palestine, found their health and adjustment generally good, and noted no special features about their adjustment period other than increased need for sleep; but he did also record that 44 per cent of them felt more nervous than in Holland, whereas only 12 per cent felt less nervous, the nervousness being inversely related to degree of success (86). The question, of course, is whether this self-assessment of nervousness has any relevance for mental health.

In respect to internal migration, information is somewhat fuller, since four main surveys mention it. Bremer's north Norwegian community study shows that 'immigrants' had a significantly higher rate of mental abnormality than 'natives', the difference lying wholly in the under-40s (9). This apparently contradicts (or corrects) Ødegaard's hospitalization finding on internal migration in Norway. But it must be noted that Bremer states his 'immigrant' group to be an abnormally selected one, thanks to local wartime conditions, with many of the disturbed coming from Finmark—a region of Norway with the highest mental hospitalization rates (64). Whether Bremer's findings are valid for present purposes is therefore doubtful, and the survey in nearby Finland reverses the position again. There, in the 1930s, it was found that the province with the highest inward migration (Viipuri) was the one with the lowest rate of mental disorder, this last term excluding

mental deficiency and epilepsy (45). No age adjustments were made in that study, which was based on hospital plus key informant sources, but if the migratory trend was, as is the more usual, one of adults rather than of families with children, then correction for age should increase the difference in rates rather than reduce it. Of course what is offered here is only indirect evidence, and it could easily be that the province which attracted most migrants was the one which possessed to the highest degree those features which benefit mental health. By itself, therefore, one cannot put much weight on this survey. The third study, that on the Eastern Health District of Baltimore, produced the oft-quoted conclusion that "There is a definite inverse relationship between the prevalence of mental health problems and duration of residence in the house, but no such relationship can be demonstrated with length of residence in the city." (80) This seems much more direct evidence on the matter, but Dorothy Thomas has pointed out (55) that the method of conducting the survey was such, probably, as to exclude from final consideration the most mobile section of the population. Finally, Gartly Jaco's Texas study (43) offers similar negative findings, but it is open to criticism from another source. He found that there was no significant difference in psychoses rates between those born in the state and those born elsewhere, whether Hispano-American, Anglo-American or 'non-White', but his data only cover psychoses here, and his method of survey excludes those who had not sought treatment. There could thus be a marked difference in neuroses rates, or in the rates of untreated psychosis, which this particular method of approach would not show.<sup>5</sup>

<sup>5</sup> A fifth study of great relevance was overlooked in the first draft of this paper. Martin, Brotherston and Chave investigated the incidence of mental disturbance in a new housing estate [=development] outside London, using hospital admissions, psychiatric outpatient clinic visits, general practitioner consultations, and sample interviewing of some 750 families. A nonmigrant control group was not studied, but from comparison with previous studies the authors conclude that virtually every indicator of mental disorder used showed the new estate dwellers to be less mentally healthy than expected. They are undecided whether to attribute this finding mainly to the social change or to the relative isolation of life in such a development, but they note that symptoms were more frequent in the most recently migrated than in the longer

(Continued on page 409)

With none of these surveys being satisfactory for present purposes it is to be hoped that the Stirling County and Midtown studies—neither of which have been published in adequate detail at time of writing— will help clarify the situation. All that one can say is that present data, none of them satisfactory alone, nevertheless do tend to suggest that mental disorder is not increased in migratory populations.

If there is no mental health survey on the transition from peace to war known to the writer which seems useful to cite here, there are two on cultural change in non-Western peoples. The better known is that of Tooth on the Gold Coast (81), who sampled a number of districts for cases of major mental disorder revealed through census or by local chiefs, but who is quite doubtful about the representativeness of the result. He found a significantly higher rate in one district with four large towns in it than in the other districts, and gives many clinical descriptions of cases in which Western influences may have had a precipitating action, but he regards the difference in incidence as reflecting nothing more than differences in ease of ascertainment. He concludes that "... this survey provides no evidence in support of the hypothesis that psychosis is commoner in the Westernized group than in the rest of the population . . .", although "... it may be that among the neurotics and minor forms of personality disorder the exposure to Western culture has a more unsettling influence." Carothers, commenting on this finding, suggests that it might be due to a greater mortality among the non-Westernized insane group (so that a difference in incidence might exist even though no difference in prevalence) but continues to insist that what is remarkable is not the relative levels in Westernized and non-Westernized Africans but the low overall level of mental disorder in Africans whether they are Westernized or not (14).

resettled. Without presenting data they suggest that "psychological maladjustment was exceptionally common among children on the estate immediately after rehousing, but that a large measure of stabilization occurred later." A local, subcultural, factor in this case may be the strong clannishness of London East End families, which the rehousing disturbed. [56]

The final survey to be cited here is one in which the level of prevalence was by no means low, and where a form of change does possibly seem implicated, though whether one should take it as representative of social change in general is very questionable. Van Loon followed up a psychiatric census of North Sumatra in 1918 by visiting and examining patients in different sample areas, and came out with very interesting data (84). He found that major mental disorders were extraordinarily frequent in certain areas and quite infrequent in others although the high prevalence areas were precisely those in which one would have expected the poorest ascertainment, since they were at the heart of a culture which had recently been defeated by the Dutch after a long war, whereas the low prevalence areas were those in which the Dutch administration (and plantations) had been most quietly welcomed. Further, a re-analysis<sup>6</sup> of his data (44) shows that in the high rate areas, male cases greatly outnumbered females (110 to 40) and were predominantly schizophrenic although an impressionistic medical report on the same people made when the war was in its early stages states that mental disorder was infrequent, occurred mainly among females, and was mainly of an acute confusional type. If the earlier report is accepted, then it seems highly probable that there had been an increase in male schizophrenia and male mental disorder generally, between the time when these people were apparently successfully resisting the Dutch and the time when Van Loon saw them as a defeated tribe whose males had lost their *raison d'être*. (Apparently the Dutch did not recruit them into their own local army, as the British had successfully done after the Sikh rebellion, and Van Loon describes the males as listless and inactive, while the females had taken over the running of affairs.) This seems a possible example of mental disorder increasing in response to social change, but one of a special character. Not only was this people suffering military defeat (not in itself necessarily a precipitant of mental disorder, as we know from elsewhere)

<sup>6</sup> Given in full in reference (61).

but for the lifetime of the current generation the persistence of the war had meant that the formerly successful warrior ethos could neither be quietly abandoned nor be actively tested against various more adaptive alternatives. These males could, therefore, be considered to be trapped by the impossibility either of succeeding in terms of their traditional culture or of finding an alternative field to succeed in.

#### PSYCHOLOGICAL TEST EVIDENCE

The picture which has developed so far is, despite all its complexity, fairly clear in one major respect. There are almost as many studies which suggest that social change leads to *no* increase in mental disorder, or even to a decrease, as there are studies suggesting that an increase is directly traceable to such cause. This would be satisfactory backing for the belief that social change has an adverse effect when in combination with certain other factors but no adverse effect in general, were it not that the minor mental disorders have scarcely been dealt with at all. Not only the hospitalization studies, but the mental health surveys which have been cited have dealt quite disproportionately with psychoses only, or frank 'insanity.' Yet the few times the lesser disorders have been mentioned there has usually been some impression that they were frequent in the groups experiencing change even when the psychoses were not. They were the main part of Bremer's Norwegian migrant disorders; they might be inferred to be slightly raised in Weinberg's Jews; they were exceedingly frequent in some refugee camps even when the mental hospitalization rate was low; they may, according to Tooth, be raised in the Westernized African. Moreover, such disturbances have been reported as excessively frequent in Asian and African students at Western universities;<sup>7</sup> in the Christian Bataks as opposed to the pagan Bataks;<sup>8</sup> in ex-P.O.W.s (17); and in certain groups of immigrants (60). Evidence of the latter kind is

<sup>7</sup> Personal communications from various student health services.

<sup>8</sup> Personal communication from Professor P. M. van Wulfften Palthe.

not all in the one direction. There are, for instance: Aubrey Lewis' finding that neuroses did not increase in Britain during the first years of World War II except in those who had been bombed (51); reports on the disappearance of neuroses in persons confined in concentration camps (often with their reappearance after release (8, 74)) and Russell Frazer's wartime finding that "Workers who had during the war changed their residence or their work, often under compulsion, had no more illness than the rest" (28). However, the latter evidence tends to be associated especially with war, and it might be argued that the changes which war brings are not permanent and hence do not have the same effect as other changes. Some further exploration is therefore desirable with regard to lesser degrees of mental disturbance. If, in general, certain symptoms were regularly found associated with social change then the question would still have to be raised as to whether these represented a pathological state or a transient and essentially healthy adjustment phase (for instance, it might be asked whether serious personal loss can be healthily adjusted to without some depression), but that need not be dealt with before its time.

Mental health surveys do not provide more than what has already been mentioned, but a source of checking does exist in population samples that have been given psychological tests of the projective or symptom-list type. The validity of such tests as measures of mental health is still debated, but when the same test is given by the same administrator to two or more groups having different exposure to some variety of social change, then the results are worth examining. Since it is a marginal area of the present subject, however, the literature has not been searched for all available evidence and only sufficient examples will be cited to indicate on what side of the question such evidence is likely to fall.

Studies do exist where a group having seen much social change is shown to be less healthy than some comparable group which had experienced less change. Hallowell's Rorschach stud-



ies of different groups of Ojibwa, for instance, showed that those which had made the most rapid strides towards apparent acculturation to modern United States society showed signs of regression or "frustration of maturity" which less acculturated groups did not (36). However, it is relevant to note that the apparent change made by the more rapidly moving had no depth, and that true acculturation to modern society was being frustrated by white attitudes. Another example is Grygier's study, using a battery of tests, of Polish and Jewish displaced persons and KZ survivors immediately after the war (35). His findings suggest marked psychopathic traits in this group; but, of course, traits which suggest psychopathy in peacetime were those which it had been necessary to acquire for survival in Nazi-occupied Poland and it took some time to discard such traits after the war ended.

Opposed to these studies and more relevant for present purposes are some which show no apparent difference in mental health as between two relevant groups. One such is the Algerian work of Miner and DeVos. They compared residents of a small and relatively isolated oasis in the Sahara with people living in Algiers who had been born and brought up in the same oasis (21). The mean Rorschach scores for the two groups are significantly different in a number of items, but these differences relate to the modes of expression of maladjustment or adjustment only. The 'maladjustment index' was much higher for the city Arab than for an American 'normal' sample, being as high as an American neurotic sample gave, but the oasis Arab group was not any lower. The city Arab showed indications of aggressive feeling which were presumed to be related to his situation *vis-à-vis* the French, but the oasis Arab showed disturbances in a different direction, some of which the city sample appeared to have less of. A second study is a Rorschach one of Spanish and English children, the Spaniards being refugees from the war in Spain and the English, evacuees (83). Differences were again found between the groups, but again these were traced to their different cultural backgrounds;

the Spaniards' presumably greater experience of change is not revealed in any signs of greater suspect pathology. A third example is the writer's own work with Malayan students, using Sentence Completion Tests, Rorschach, and some other so-called 'tests of neuroticism'.<sup>9</sup> At the present stage of analysis no signs of greater mental disturbance can be found in the tests of students coming to the British-style university from up-country towns and villages in Malaya than in those coming from the three main centers of population and Western influence. This result may be affected by the fact that it probably needs more intelligence and drive for a village boy to reach university than for a city one, but at time of writing this factor does not seem to have been relevant.

These illustrations suggest that in as far as psychological tests are able to reveal lesser degrees of mental disturbance, such lesser degrees seem to be no more consistently raised in the presence of social change than are the major disorders. The predominant impression is that they may be raised in association with change in certain contexts, but not significantly raised in others.

#### SUMMARY REGARDING THE COMPETING HYPOTHESES

A backward glance over all that has so far been reviewed is now in order. What we have found is that in one type of social change situation, and one only, there is fairly unanimous agreement that those undergoing rapid change show more mental pathology than others from the same background undergoing less change. This situation is the Westernizing of non-Western peoples. However, serious doubts can be expressed whether the added pathology should be attributed to cultural change as such, or to the nature of the state towards which the change is directed, since those people who are the embodiment of that state have, apparently, a level of mental ill health which is higher still. In all the other types of situation which we have considered the evidence is more than ambivalent; it suggests

<sup>9</sup> Unpublished.

strongly that mental health, as measured by the indicators forced on us, may be worsened in some situations of change but bettered in other situations. The difference between these two classes of situation does not seem to relate to the degree of change being experienced. Possibly it may in part be related to the speed of such change, but mainly the difference seems to relate to factors which are virtually independent of the fact of change itself. Accordingly, I think we can say that the sum of the evidence is strongly in favor of the associated factors theory, and strongly against the theory of social change as a general mental health hazard.

Given that conclusion, the question now arises whether any general rules can be formulated regarding types of situation where change becomes traumatic.

#### ASSOCIATED SITUATIONAL FACTORS

*Personal Factors.* An unusually consistent finding in the literature on mental disorder and social change is the fact that there is more likely to be an excess of mental breakdown in youth and in old age than in the years of adult maturity. In Ødegaard and Malzberg's early studies of immigrants it was found that the greatest excess in hospitalization was among those aged 20-29, and in those over 70. (57, 63) In Singapore a proportionate excess of immigrant over native-born admissions was probable in the age group 15-25, and again in the age group 55+, while in the intervening age group immigrants probably produced less mental hospitalization than the native-born (61). In wartime Norway it was again the old and the young who showed a relative rise in admissions, as compared to peacetime, whereas admissions in the middle adult years showed a relative decline (65). Apparent exceptions do exist to this general pattern—Bremer's 'immigrants' (9) and the Okinawans in Hawaii (47)—but these may be due to particular local conditions. Youth and old age do seem to be factors whose association with social change is more than usually likely to be traumatic.

On the other hand, childhood appears to be unusually immune to pathological sequelae to broad social change. The children of immigrants are reported to show less delinquency (at least, in the U.S.) than the children of native-born parents (72). Wartime displacement in Britain was, as a general phenomenon, reported to be surprisingly little associated with childhood disturbances (83). Regarding the refugee status, Poslavsky and Wiegiersma found themselves unable to report any detrimental effect being manifested in D.P. camp children even though they had set out specifically to find this, and even though the amount of mental disturbance in the adult inmates of these camps (who were by this time, 1954, a highly selected remainder) was high (68). It is a broad finding from many forms of change that children remain undisturbed provided only that they remain within their family and that the family, in its functioning, does not change. What children are quite easily disturbed by, however, is major change within the family (or family substitute) itself. Disturbances in children in wartime Britain were nearly always associated with the breaking up of the family, as when the children were evacuated while the mother remained behind (24). Hospitalization of a child, as is well known today, is similarly hazardous (7, 22). Moving to a suburb from a metropolis is, according to R. E. and K. K. Gordon, mainly disturbing to male children, not female, for the apparent reason that the father must spend much more of his time away from home and hence deprives his sons of a necessary model (32). Refugee children who had lost or who had been separated from their parents were, on average, much more disturbed than those who managed to stay with them, and of those who were separated from their parents but who managed to form gangs, separation from the gang proved a disturbing action which the promise or actuality of renewed home life could not negate. (60)

This contrast between childhood's relative immunity to harm from certain types of social change and relative vulnerability to other types is not resolvable by saying that the

parent-child relationship is a unique one that cannot be put in the same class as other forms of social relationship or situation. The youth of 20 still usually has his parents, but he has become much more vulnerable than the child of 10, and the child of 10 who has lost his parents but found a peer group milieu is quite vulnerable to a disturbance of that milieu. One could say, of course, that this proves any attempt to group different forms of social change together to be pointless, but that would be, in my opinion, to go too far. An interesting, general, explanation can be hypothesized through the assumption that social change only has the possibility of creating mental disturbance when it occurs within what the individual perceives to be his 'own' or 'true' society. As Lois Murphy has said in another context, "... when an individual feels himself part of society, he may not be exposed to shock as long as the part of society which he is incorporated in, and is part of, is undisturbed." (62) To a baby, the 'true' society which it can perceive itself to be part of may consist only of the mother and itself; to a child it may consist only of the family; to an adult it may consist of face-to-face contacts only, or his whole nation, or may even be largely imaginary. Which is the perceived 'true' society will depend partly on the personality, partly on what society teaches. It seems probable, however, that with education and experience what one perceives as one's 'true' society enlarges, and hence makes one more sensitive to changes in the wider society around one (sensitive not necessarily with the meaning of vulnerable) while at the same time making one less intensely sensitive to changes in one's most immediate circle.

This factor of personal perception of society is one which, if valid, might explain not only the observations on childhood just considered, but a number of other findings. For instance, it might be the explanation why there has been reported relatively more disturbance in P.O.W.'s after their release than when they are still in captivity. One could hypothesize that the internee or prisoner-of-war, on first entering his camp, does

not regard his 'true' society as disturbed, since that 'true' society still exists in his home and in his mind; and the change, though objectively great, has thus an external character for him. (The same would be true for many recruits entering a military unit.) However, by the time these individuals are released, it seems likely that their perception of society will have shifted so that the camp community or unit itself becomes their 'true' society, and when disbandment of the latter occurs they can no longer regard this 'true' society as continuing to exist undisturbed elsewhere. The dispersal of the community is a serious disturbance of their 'true' society, as they now perceive it, and one which is too obvious to be denied. Hence restitution to their former society, which on common sense terms should bring ease and joy, would on this hypothesis be a social change whose impact would be fully felt. This, I believe, is part of what Curle has described, and the fact that the "Civil Resettlement Units" were so successful can in part, though only in part, be ascribed to the fact that they helped merge the one society into the other (17).

Other findings where this factor of personal perception might be at work are the Baltimore, Texas, and Singapore reports on migrants. It can be hypothesized that these migrants did not regard their neighbors as important members of their 'true' societies, whether they had intercourse with them or not, and hence that moving out of reach of these neighbors was not a social change in the perceived sense. The migrant to Texas may—and this is only hypothesis—be regarding his 'true' society as comprising the whole American nation and hence be finding that one neighbor can substitute for another provided only that he is American. (In overseas communities it has been reported that Americans are particularly anxious to be beside other Americans, showing more clannishness than many other expatriate peoples and getting disturbed when they must live among non-American neighbors.) (2) The Singapore Chinese, almost certainly, regards his neighbor as of little importance compared to members of his great family or clan, plus its con-



nections, and he is thus likely to perceive his 'true' society as being unchanged provided that family is intact and there are members or connections from it within reach.

More concrete support for a belief in personal perception of society as a factor is to be found in the Cornell medical histories study, to be discussed below (39). However, there are also instances where the factor does not seem to be relevant or where its existence would even seem to be disproved, unless some factor could be evoked. The Okinawan, who has such high rates of mental hospitalization in Hawaii although low ones in his homeland, should be as able to retain a mental image of his 'true' homeland society when he migrates as the Japanese can. The East Indian who migrates to Singapore and Fiji and shows comparatively high rates of mental hospitalization in these two places (61, 5) should be as able to manipulate his perception of his 'true' society as the East Indian who goes to South Africa and to British Guiana and there produces, relative to the local and white populations, quite low rates of breakdown. (50, 23) Hence while perception of society is one personal factor which probably works together with social change to produce or to prevent mental breakdowns, it is by no means the only concomitant we must look for.

The deterioration of faculties in old age is another personal factor of some relevance. It is probably the major one affecting the earlier finding that when change does produce an excess of mental disturbance, that excess is chiefly to be found in old age (and in youth, a point to be discussed shortly). When the faculties for meeting the problems which social change can bring are reduced, then naturally it seems probable that some disturbance will be produced. However, while the point must be granted, it must be noted that the expected increase in disturbance does not always appear. Bellin and Hardt have shown that loss of the marriage partner is not necessarily a factor in the mental health of old people in the United States (4) although this must surely be perceived as social change by the subject. Again, in Canada it has been shown (although I find

it difficult to follow the mathematics) that while certain forms of geographic change do seem to lower mental health, the act of retiring from work, which means a marked change on one's face-to-face society, does not affect the particular index that was used at all (11). Finally, while immigrants over 55 in Singapore probably did have an excess of mental hospitalization as compared with the native-born, it seemed even more probable that those who belonged to extended families or to a particular type of club did not show this excess (61). These exceptions suggest that the factor of senile deterioration is not in itself so important in relation to social change; there must be some intervening variable and this variable does not seem to be personal perceptions.

A third personal factor consists in the way in which the change is met. Both Weinberg (86) and Mezaros (58) have noted that the greatest degree of failure and of mental disturbance in their samples was to be found in those of a passive personality type, even though in the Hungarian group (Mezaros'), more active personalities might show more apparent disturbance in the early phase. From Ravn's report on the breakdown occasioned by the German surrender in Denmark the same conclusion can be drawn: it was the passive type of individual who had become involved in political matters, rather than the active collaborator or resistance worker, who predominated among his patients (70).

No other personal factors present themselves obviously in our material. Inherited traits and prior disturbances are probably relevant, but not necessarily so. It is easy to record that some suspicious circumstances were found in the life and family histories of patients who broke down while exposed to social change, but such writers never show how frequently the same type of circumstance appears in those who break down without exposure to change, or who are exposed to the relevant change but do not break down. On the other hand, in at least one type of social change it has been stated that it was disproportionately those with no family or personal history of predis-

position who broke down (67). Sex does not appear to be a factor. Intelligence probably is but has never been demonstrated to be so. The reason for the extra vulnerability of the young adult, noted above, may be a personal matter, but my own inclination is to see this as a matter of social roles, social expectations, etc., and hence belonging more to the next section.

*Social Factors.* One important social factor which I think can be deduced from the studies on old age mentioned earlier as well as from other sources, is that of social expectations. When an old person experiences social change and his faculties are not such as to permit him to change himself, his behavior may become inappropriate to the new situation. This, however, need not result in mental disease unless there is a social demand or social expectation that he must change himself and that his behavior must be socially appropriate.<sup>10</sup> If society recognizes the old person's limited ability to change or accords the old the privilege of not conforming (as happens in some degree in cultures such as the Chinese) then it seems probable that social change as a general experience will not be traumatic. Similarly, if the young man were permitted to adjust to change at his own rate and not expected to respond in an adult fashion before he has the knowledge of life that an older man has, then we might find this age group would show no greater vulnerability to change than any other.

Social expectations are important here not only—and not mainly—with respect to age roles, however; they probably are a significant factor in quite a number of other situations we have been considering. The expectation of acculturation with which immigrants to the U.S. were formerly met, and the absence of such expectation in Singapore, is probably relevant to the native-immigrant rate differentials in each country and possibly also to the shift which appears to have occurred over the years in the United States itself. Similarly, the changing

<sup>10</sup> Support for this assumption is reported to be accumulating in the Duke University Studies on Mental Adjustment in Old Age (Professor E. W. Busse).

expectations which the Negro section of the United States population has had for its members are probably relevant for the finding that Negro rates of mental hospitalization in Virginia have been rising more rapidly than white rates even though Negro social rights and status have improved (89).

Expectations are not all, however. There are other factors which seem frequently to play a part in making change a hazard, and still others which seem able to make change beneficial (an effect which is often ignored).

Closely allied to the factor of social expectations is that of social assessment. The Okinawan in his homeland is an individual among other Okinawans, assessed by his neighbors according to personal and family traits and history. When he leaves his homeland, however, he is likely to come in contact with a Japanese-originated belief that Okinawans are *Untermensch*, an opinion which is distasteful to him, which he cannot wholly reject, which he has not been brought up to accept, and which poses a problem of self-restitution that has no easy solution. To this extent he stands at the opposite extreme to the Japanese and Japanese-American migrant to the Chicago area during the last war. The latter possessed cultural traits which were falsely identified by the surrounding American middle-class as ones to which prestige was customarily given. He thus not only found it easy to be accepted (where there was no prior prejudice against him) but from the environment's perception of his traits tended to acquire opinions regarding his own status which were gratifying and which counterbalanced such frustrations as the new situation brought.<sup>11</sup> Hence he is reported to have adjusted better than some European groups and, at least judging from the TAT reports, was not mentally disturbed (15). Probably this mechanism of identification with society's assessment of one's group accounts for much of the marked difference in mental hospitalization rates which the East Indian demonstrates in different countries,

<sup>11</sup> This is Caudill's own conclusion; the impression that the mental health of the group was also good is the writer's.

for in Africa and British Guiana (23) they had a mass of people to whom, following the predominant white attitude, they could regard themselves as superior—the Africans and Amerindians—whereas in Singapore, Fiji, and some other locations where high Indian rates have been suggested, no such reserved status exists for them.<sup>12</sup>

The Japanese-Americans were aided in meeting change by what Caudill regards as social misconceptions about themselves, but it seems reasonable to expect that deliberate efforts by society to assist the individual to adjust to some change would have a similar or even better effect. One interpretation of schizophrenia is that it constitutes a pathological solution to a life problem, and on this theory it would seem probable that where tradition or current society taught, and did not obstruct, a successful and healthy solution to whatever problem a particular social change has brought, then no rise in schizophrenia would occur. Similarly, it has been suggested that immigrants are more liable to the CASSP group of disorders because they must make a greater effort during most of their life to attain customary ends—effort at problem-solving one might call it. If this were so, then again it should make a great difference to the risk of getting such disorders if tradition or current authority teaches clear ways of meeting a particular problem. Conversely, however, if society avoids defining a problem which change has brought (and not only new problems, of course) and avoids discussing the possible solutions, then the risk of pathological solutions or of excessive effort seems much greater.

An illustration of this difference relates to the British P.O.W. When their resettlement problems tended to be ignored by the home communities the result was that relatively many got disturbed, but when their problems were tackled in "Civil Resettlement Units" there resulted considerable improvement

<sup>12</sup> It might be asked why whites in the tropics tend to have raised rates of breakdown, since they have even more people to look down on. A partial answer is that where social standing can be taken for granted, with no sense of the 'white man's burden,' then rates are low, but today the White is usually too ambivalent about his right to higher status to gain any benefit from it.

in the subsequent mental health of those P.O.W.'s who had passed through these units (61), an improvement which might in part have taken place even if the interest and aid had assumed quite a different form. Another illustration comes from the Indian Army, where a low level of mental breakdown was noted in soldiers coming from subcultures where the power of the patriarch or local leader was traditionally strong. Here the previous pattern of submission to the chief could easily be converted into submission to, and trust in, their officers. In contrast, much higher rates of breakdown were found in Indians coming from the South, where local leaders have less tradition behind them and the patriarch, if there is one, tends to rule by force or through joint consent rather than by simple acceptance of his authority (88). A third illustration can be seen in the finding that Malays in Singapore had high rates of mental hospitalization when they followed a commercial occupation, whereas this occupation was associated with notably low rates in the Chinese and Indians. The difference stemmed, in part at least, from the fact that Malays had no tradition of commercial competition, while this did exist for the others (61).

It will be appreciated that the social factors which are being mentioned now are not ones which appear exclusively or predominantly in association with change. The image which society has of an individual, the expectations which it holds out for him, the aid which it offers in the solving of problems—these are all factors which affect mental health whether in company with social change or without it. The final factor which will be mentioned here is similar: it is the values which society puts on different experiences and actions. It must be admitted at this point that all the social factors which have been mentioned are ones which are not easily amenable to identification, comparison, or measurement by traditional epidemiological techniques, and to move from the assessment of age as a factor to the assessment of values as a factor calls for a great technological jump. The next study to be mentioned, however, illustrates one direction in which the jump might be attempted.



In the writer's opinion it is through similar attempts that this side of the epidemiology of mental disorder will have to advance.

The work just referred to is the Cornell medical life histories study led by Hinkle and Wolff (39). One of the main conclusions which they offer has considerable relevance here. They say: "The great majority of the clusters of illness episodes that occurred in the lives of every group occurred at times when they perceived their life situations to be unsatisfying, threatening, overdemanding and productive of conflict, and they could make no satisfactory adaptation to these situations." An important inference from this is that where a change in life situation occurred and a satisfactory mode of adaptation was discovered, no particular excess of illness developed. (It must be an inference at present, for in none of the published papers on the project's findings is this specifically demonstrated to be true.) Another inference, this time drawn from individual case history diagrams which they have published, is that where such a cluster of illnesses is occurring social change may result in its cessation and hence in improved health. However, of still more relevance is their finding that "The relevant variable . . . is not the 'actual' environment and the 'actual' experiences themselves, but the subjects' perceptions of these." In other words, it is not social change, but how such social change is perceived, which is relevant for health, and while such perception is to some extent idiosyncratic, to a large extent it is determined by what society teaches. When Herodotus has Darius invite the Greeks to eat their dead fathers instead of burning them and the Gallatians to burn their dead fathers instead of eating them, and has him receiving horrified protests from each, he points a moral which is still surprisingly often forgotten today, namely the social or cultural relativity of values. Many of the life situations which are perceived as "unsatisfactory," "threatening," etc., by individuals in one society would not be perceived thus by people in another. To take the simplest examples: The introduction of modern contraceptive methods to

non-Western peoples may in one case result in considerable relief in mental distress and in another case result in considerable augmentation of such distress, according to how the prevention of pregnancy is viewed by the different traditions. The discovery that his wife can make more money in New York than he can would probably have a very different impact on the mental health of a Southern Negro migrant than upon that of a Puerto Rican migrant. In Singapore, to have many possessions was of importance to the Chinese since it helped the perpetuation of the family, but not to the Malay, whose values were much more focused on emotional gratifications than upon security and material gratifications. The Chinese showed a marked inverse correlation between mental hospitalization and social class; the Malays showed none (61).

#### TYPES OF BREAKDOWN PARTICULARLY ASSOCIATED WITH SOCIAL CHANGE

Although social change does not have a pure effect on mental health in general, it does seem to be associated with certain types of breakdown. Of these the most notable, because it is a condition which is otherwise rather rare, is the acute confusional state. This has been described as especially increased in students in their first year at college (13), in immigrants just arriving off their ship (63, 60), in recently inducted soldiers (48), in foreigners lacking a knowledge of the local language (46), and as being increased in a number of countries (including Norway) (65) during a war. Depending on circumstances it may take on a paranoid or depressive coloring, but its especial characteristic is the good prognosis. Carlson and her colleagues have suggested that it may develop as a defense against rage, this rage developing in response to the insecurity which change brings to prior conflict situations (13). It appears most commonly in the young, in the uneducated, in those who are alone; and as a generalization one might say that it appears especially in those who have not been taught, or have not learned, techniques for meeting new situations. Associated factors are physi-

cal illness or debility and lack of adequate means of communication. When properly handled it disappears quickly, and prevention seems quite possible through the provision of better guidance for the meeting of the new situation.

Another even rarer condition, but probably more closely associated with social change than the confusional state, is epidemic hysteria.<sup>13</sup> The majority of instances of this condition included by Gruenberg in his recent article on the subject (34), and all other instances known to the writer, could be said to have appeared subsequent to some social change affecting the community. For instance, the first great burst of epidemic states in convents that Calmeil has recorded (12) occurred throughout Christendom between 1550 and 1600, just at the time when Europe was being shaken by the Reformation, Counter-reformation, and by the changes within the church which brought these movements forth. The Vailala madness was a clear response to the demonstration of Western power (34). The outbreak of pseudo-epilepsy in Yugoslav partisans occurred when war pressures were released and readjustment to the problems of peacetime was called for (66). Often such outbreaks can be interpreted as socio-pathological solutions to some problem which tradition prevents being solved in a more adaptive fashion and the others, as in the Yugoslav case, can be interpreted as mimetic individual efforts at an individual solution.

Among the more familiar conditions, mild depression, schizophrenia, and arteriosclerotic psychosis have all frequently been reported in association with social change, but it should be noted that change can also cause a reduction in their frequency. Depression, for instance, has been described in ex-P.O.W.'s, in internees, in immigrants, and in those away from their country on study tour (52), but if one were to take types of change which lead to an addition to an individual's 'true' society rather than to a temporary reduction of it, then the reverse influence

<sup>13</sup> Hysteria is not necessarily the best word here, but it is the most commonly used term.

should logically appear. Schizophrenia tends to be increased in association with social change where there is an additional expectation to adapt to the change and no clear guide as to how to do so, but in wartime Norway manifest schizophrenia was reduced, possibly because expectations regarding social behavior were also reduced; and in Singapore immigrants had less schizophrenia than the native-born probably in part because no new expectations were imposed on the former. Whether arteriosclerotic psychosis can be similarly reduced by certain types of social change is less easy to say, for at the present time the general direction of social changes is to impose new social burdens on man rather than to relieve him of them, and one theory, at least, proposes that additional mental burdens means additional risk of this condition. Since the effect is presumably a delayed one, research into the question is not easy, but some of the Singapore findings with regard to membership of extended families and of social clubs suggests that this condition is considerably preventable where the change (in this case immigration) is accompanied by an increased sharing of responsibility among adults.

The paranoid tendencies which have been described in certain types of European immigrant are not sufficiently affected by other forms of social change to be specially featured here.

#### GENERAL CONSIDERATIONS

There are a number of questions pointed up by this survey which call for investigation in the future. The first noted was the degree to which other variables like occupational status or self-selection were affecting the data on migration. A second was whether the apparently heightened rates of mental disorder in Westernizing sections of non-Western peoples was related mainly to the process of cultural change, or to the nature of the Western culture towards which the change is directed, or to other aspects of the situation as the deliberate frustration by those in power of efforts to become Westernized. A third question was the definition and investigation of the vari-

ous social factors such as expectations, value systems and indoctrinated modes of social perception. Techniques for providing an answer to the first question are known. Answer to the second must probably depend on the collection of sufficient mental health data from the numerous locations where such cultural change is taking place; but this again calls for no particularly new techniques. The third question, however, requires types of information for which new methods of collection and new standards of assessment are required—new, at any rate, in epidemiology. While the Stirling County study and the Cornell life histories project offer some guidance as to means of collection, means of assessment are still largely beyond us and it is questionable whether the social sciences are better placed to guide us. It is to them, however, that the problem should initially be handed.

The corresponding problem in medicine that arises from this survey is the assessment of superior health. In discussing the effect of social change on mental health it has been necessary to concentrate, virtually exclusively, on the presence or absence of disease. However, social change, and almost all the other types of factors with which this conference deals, can have two types of effect: it can shift the whole spectrum of health to the right or to the left, and it can also—at least theoretically—broaden or narrow that spectrum. If we only consider the possibility of a shift to right or left, then shifts noted at any part of the spectrum should be valid for the whole of it. However, should the other type of effect be admitted, then measurements in any single part of the spectrum are no longer unambiguous: a shift to the left in a part of the left-hand side of the spectrum may mean a total shift to the left (e.g. a general worsening of mental health) but it might also mean a broadening of the spectrum with the center staying where it was (i.e. an increase in both the most unhealthy and the superiorly healthy). It must always be medicine's task to reduce the amount of disease, but it makes a difference to social policy to know whether a certain type of projected hazardous change is likely to decrease

mental health generally or to increase the number of both the sick and the superiorly healthy. If the latter result is forecast, then the risk may be taken with a better conscience.

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## DISCUSSION

DR. E. GARTLY JACO: I wish to congratulate Dr. Murphy, on behalf of the members of the conference, for preparing a most stimulating paper on a very complex subject.

I would like to outline what I think Dr. Murphy is presenting and then I would like to add, if I may, some other suggestions related to this topic. As I see it, there are two major ideas or theories expressed in this excellent paper. The first is the idea that if social change itself is pathogenic, then mental illness will increase for those experiencing greater change. Secondly, if social change is pathogenic

only when combined with other factors or conditions, then these other conditions, and not change itself, should be investigated.

These are what I would call *etiologic* hypotheses in contrast to *epidemiologic* hypotheses. I will come back to this point later. There are four major factors that Dr. Murphy has assumed to be indicative of social change. One is immigration or international migration; the second is internal migration; the third is the impact of war and peace; and the fourth is acculturation to Western civilization. In almost all cases, hospitalization rates were the major quantitative data used to test these four factors with regard to mental illness. This is probably one reason why it is no surprise that equivocal and inconclusive results were obtained for all these four factors as related to social change. Hospitalization rates are unreliable indices of incidence and prevalence of disease, especially mental. As such, they are subject to distortion and bias from a variety of factors, and make poor sources of data to test epidemiological, not to mention etiological, hypotheses.

Turning to change itself, if we can accept a broad definition of social change, then we would also include under this rubric cultural and personal change. That is, social change may range from a change in a dyadic, one-to-one social relationship (such as between patient and therapist, or husband and wife, or parent and child) up to the establishment of a new religion, a new government or economic system in the total society. This means that social change is a very complex and extensive subject, should one conceptualize this kind of approach in examining its components and consequences.

I wish some other facets of change had been included in this paper. One I think to be important is industrialization, which is implied to some extent in the factor of acculturation. The Industrial Revolution has been a most significant force in changing not only the Western world but also other parts of the world, and will apparently continue to be so in the future.

Another factor would be urbanization—the expansion of the urban community everywhere, usually following in the wake of industrialization.

Also, I would regard social mobility as a very important instrument of social change, especially inter-generational mobility which has recently been receiving increased attention. Changes in the family life cycle from one type to another, such as moving from the

family of orientation into the family of procreation, and eventually into the family of gerontation (the family of old age) might develop clues of significance.

The types of mobility we call horizontal and vertical social mobility could well be an important topic. These have been discussed to some extent in some of the other papers. I am not talking here about the drift hypothesis with its dimension of spatial mobility, but about whether or not one achieves or loses social status.

For example, in my own survey of psychoses in Texas, there is one finding I would like to present to the group for discussion. This is the effect of education on the incidence rates of psychosis for certain subcultural groups in that population. I found that the incidence rate of psychosis increased consistently with level of education *only* for the Latin-American (Mexican) group, and for the non-white (Negro) group, but was not found for the dominant Anglo-American group, where no correlation between education and incidence was obtained.

If we can, as we do in sociology, accept education as a channel of vertical mobility in our society, then a member of a minority group who uses education to advance himself up the social ladder, might be under tremendous stress and it would certainly be indicative of a change in his social status. The percentage of college educated Latin-Americans and Negroes in Texas is very small and these individuals would certainly be deviants in this respect. I might add, too, that the *kinds* of mental disorders found were correlated with education in both of these subcultural groups. The most outstanding instance was the very positive correlation between the rate of high toxic psychoses and education, which stood out above all the other types of psychoses; therefore, I think social mobility is a very important aspect of social change in its impact upon the individual—the index case.

Other instruments of change could be added, such as the impact of cataclysms, catastrophies, disasters, and revolution and reform.

I would like to raise a methodological point, although realizing that it may not be a widely accepted one. I would question the attempt in this paper to use epidemiologic data, such as hospitalization rates, to test etiologic hypotheses.

I think this is a misuse of epidemiologic data because I think one should use epidemiologic data to test epidemiologic hypotheses, and



etiologic data to test etiologic hypotheses. One must re-formulate epidemiologic hypotheses for etiology and derive a different type of data to test etiologic hypotheses. The basic datum of epidemiology is the *rate*; of etiology the *case*. The former rely upon probability statements about groups or populations; the latter on "causal" statements about the individual.

I would also question the attempt to measure or to test social change in terms of personal change. For example, what happens to an immigrant as a migrating individual may or may not be indicative of social change due to migration, or to a migrating society, or to a group which loses or gains population by out-migrants or in-migrants.

I would like to discuss some other concepts of social change as they may bear on mental illness rates.

Not every component of society, as Dr. Murphy pointed out, is affected by social change occurring in that society; there is always a differential exposure to these stresses of change. This is important in evaluating the effects of change in psychopathology. It is quite possible that certain types of change may be pathogenic while other types might be therapeutic. There is one theory which holds that social change occurs because the existing society no longer is meeting the "needs" of its members. Failure to meet the needs of the group implies a pathological condition conducive to bringing about change or establishing a healthy condition where these needs are met by the new social system supplanting the old. If this theory is valid, then some types of change might be therapeutic as well as pathogenic, depending upon their consequences.

One could look at social change in another way. It might be that the factors that are conducive to change are also the ones that contribute to mental illness. Instead of change itself bringing about mental illness, those very conditions that brought about change might be the ones that affect the risks of acquiring a mental illness. I think we should look into the causes of change to see if they might also be causes of mental illness rather than look at the change itself, after its conditions have been met and its consequences faced.

What about the individual's role in social change? Are persons who are predisposed to mental illness more likely to contribute to social change than others? There have been some studies of the personalities of so-called leaders of social movements, political leaders, leaders in wartime, and others that tend to suggest this. These

people may have a predisposing type of personality that may either make them more susceptible to promote change or, because of their incipient mental disturbance or disorganization, might do something about changing social conditions for others.

One could discuss a great variety of factors in this very broad topic. One could also offer some hypotheses open to research or discussion. If we can accept the possibility that not all social change causes personal change, then not all forms of social change are likely to have an effect on psychopathology. This I put in hypothetical form.

One could raise the question: Are certain functional illnesses in themselves defenses or resistances to social change? It is quite possible that certain functional illnesses are *symptoms* of resistance to change, either change in a social relationship within the family, or in the occupational sphere, or in any other significant, meaningful, personal sphere of human behavior.

The frequency of functional illnesses, therefore, might be an index of change going on, or the amount of resistance to or defense against change in certain social spheres. Anxiety behavior certainly could reflect anticipation of an impending change, perhaps a premature effort to cope with change or to be prepared for its consequences. In this case, we are using social change in terms of an interpersonal relationship, as well as in broader areas of human behavior.

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#### SUMMARY OF DISCUSSION

1. Dr. Murphy deliberately did not define social change in his paper. The absence of such a definition, however, created basic difficulties in understanding which particular kinds of influences were being referred to. It was felt that a clear-cut definition was needed on what was meant, and what was not meant, by the term "social change." Thus, whatever the definition, everyone undergoes social change at one time or another. Yet there exist periods in the lives of individuals when the *absence* of social change might be more disturbing than its occurrence, such as when the man who, desiring a promotion which would bring with it increased social status and higher living standards, fails to get it.

In answer to this point, it was suggested that some terms did not

need a precise definition of what they referred to since they developed, instead, a proper appreciation of the framework within which they might usefully be considered. Such terms as "socio-economic class," "poor hygiene" and "social change" are convenient ways of grouping together very roughly related phenomena. They are not unitary factors but constellations of factors, so that when attempts are made to find causal relationships these terms have to be broken down into their component elements. They are useful for marking rather broad targets in order to see which particular components seem to be related to which particular kinds of disorders and in what combinations.

The concept, as a general label, helps guide and interpret particular observations, but the concept cannot be directly observed. Thus, while it can be noted that some persons do not brush their teeth or take baths, one is not observing "poor hygiene" as such. The concept forms part of the interpretation of particular observations; and these in turn form an operational definition of the use of the concept. The error is to forget the observations and to report directly in terms of interpretation—an error of pre-classification.

2. The term "migration," like "social change" is a category embracing many different types of phenomena. For any specific migration, it was important to examine the actual context in which it occurred, since at various times and places two distinct mechanisms were at work: involuntary migration (ranging from slavery and taxpeonage to political refugees) and voluntary (including mass labor recruitment and individual self-selection).

If much has been written about mental disorder in migrant populations under the heading of social change, far less has been reported about the nature of the populations that remained at home. There were suggestions that in these non-migrating populations there existed a reservoir of considerable pathology, including psychopathology, and one hypothesis proposed that it was the amount of pathology that was present that determined whether a person was able to make the decision to migrate.

3. Various methodological points were raised on the investigation of social change and migration in relation to mental disorders. The study of immigrant groups presented characteristic difficulties because they could not be considered representative samples of the populations from which they came. Usually it was not practicable

to observe how they differed psychologically or physically from their parent populations, and most studies only compared immigrants with the population of an area receiving them. Dr. Jaco's study of Mexican immigrants to Texas was such an example. He described two unexpected findings which turned up when his data on incidence rates for psychosis were adjusted for age and sex to make them comparable to U.S. born persons of Mexican parentage in Texas. While the crude rates for immigrants were higher than for the native born, the age-adjusted male rates for immigrants were lower and the rates for females were higher, than their respective U.S. born controls. Dr. Jaco offered an explanation in cultural terms: The male migrant considered immigration an achievement, while for the female, immigration meant loss of identity bound up within the traditional Mexican family structure.

As a suggestion for a possible study comparing psychiatric illness in the successful migrant with the unsuccessful, the emigration of West Indians to both London and New York raises the standard major problems: How and where is the process of self-selection to be studied? How are rates to be developed when population denominators do not exist?

4. It was noted that these inherent methodological difficulties which were involved in working with immigrants could be avoided by studying internal migration—that is, migration within a single country. As an example, coal mining areas in Britain which had experienced shifts in population over a period of time showed an interesting variation in death rates which related to those areas having the heaviest emigration. These areas, while they showed the lowest suicide rates, had the highest death rates from heart and lung disease.

These findings, while they might suggest a number of explanations, required field surveys to test out hypotheses. Cochrane, again studying coal miners, examined both migrant and remaining populations. He found that the physical condition—as measured in terms of lung function—of the miners who remained in the South Wales coal areas, was much worse than that of the men who had emigrated to new coal mining areas in other parts of the country.

5. While such clinical case studies have their limitations, they allowed solutions to the problems of measurement that are better than the usual comparisons of mortality or morbidity rates that are to be

found in broad population studies. Methodologically, it is necessary to devise a case-finding technique that is not affected by the social changes under study—namely, migration. For it would be most extraordinary that an immigrant population would have the same relationship to clinical services in the new country as it had had at home. This seems to underline the point that data on migrants derived from routine clinical contact were rarely, if ever, adequate. What appears necessary is to set up case-finding survey teams dissociated both from the migrant and from the non-migrant populations under study.

Yet to leave such social institutions as hospitals with their established reporting systems was to become mired in a swamp of diagnostic problems. In his paper, Dr. Murphy acknowledged that the methodology that had produced existing data had generally been so crude that it was not really possible to establish the facts. So that, were he to have included an expanded definition of mental disorder which encompassed such forms as delinquency, or general anxiety, or the entire series of presumably psycho-physiologic disorders listed in the APA nomenclature, he would not have been able to use even the crude measuring stick of hospital admissions. For there existed no standards of comparison; not even a methodology for arriving at a standard. It was a problem that had long engaged such conferences as these: What was mental disease? What was a case?

6. Replying to the last two questions, Dr. Leighton reviewed some studies that he and his colleagues at Cornell had been working on. From these has come the concept that every community—viewed as a socio-cultural system or quasi-organism—has a “threshold of tolerance of change” which is related to the *amount* of change (whether benign or not) that occurs within a certain length of time. When the rate of change extends beyond the threshold of tolerance, the community begins to fall apart, failing in such functions as the socialization of children or the maintenance of economic enterprises. While some disjunction occurs with any change, the threshold is that point at which the changes become so great that they develop into a vicious circle—a spiral of more and more disintegration—independent of the factors that induced the changes in the first place. It is believed that the population of such communities would, in trying to cope with their situation, develop more mental illness than the population of communities below the threshold. The fundamental prob-

lem, as has been noted, is to find standards of comparison that would hold say, for villages in such disparate places as Burma, Japan and Peru.

Two possible frames of reference were suggested for this problem of cross-cultural comparisons of psychiatric disorder. One would be to use the psychiatric definitions now current in the European-American cultural system to see what patterns of disorder existed. Such an approach does permit appraisals without too much difficulty of such conditions as mental retardation or even schizophrenia. However, to attempt to assess personality disorders, psychoneuroses, and sociopathic behavior, would raise some very real problems. The other frame of reference would be to adopt the definitions of deviant behavior that are native to the culture under study—to work with its own concepts of what is queer, strange, abnormal, etc. In practice, Dr. Leighton concluded, the best course was to develop criteria using both frames of reference.

7. The opinion that epidemiological data was not suitable for testing etiologic hypotheses, was disputed. In support of this view it was maintained that, while actual causes operating at the level of the individual case were reflected in group data in the form of rates or probabilities, these data could only suggest hypotheses of cause and effect but do not constitute a test of such hypotheses. In opposition, it was maintained that epidemiology, as the study of occurrence of disease, is necessarily concerned with disease etiology. Classic epidemiology studies, such as Baker on Devonshire Colic and Goldberger on pellagra, were cited as instances in which etiologic knowledge was gained through the epidemiologic approach.

8. Dr. Murphy was asked to discuss the statement he made in his paper that "... in wartime Norway schizophrenia was reduced, possibly because expectations regarding social behavior were also reduced. . ." What was the evidence for this reduction, and what was the evidence for the imputed cause?

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DR. MURPHY: There are three main strands, I think, in the discussion. One is the question whether we can usefully have any discussion at all when we do not have definitions of the topics being discussed. The second is methodology. And the third is the question whether social changes are useful things to generalize about at all.



On this last point, the aim of my paper was to see whether generalizations about social change seemed useful or not. We always seek the widest generalizations possible in research in the hope that findings from one area may thereby be made useful in others. There are plenty of people in the past who have thought of social change as encompassing all the various things I have referred to, but this does not mean that the concept is useful to us. Perhaps bringing them all under the one rubric is wrong. I have not, myself, come to any answer on the point. However, the general approach is the one which we customarily use, and I think it is the one which we should continue to try and achieve, by seeing what degree of generalization is possible and what goes too far.

Now, as to definition: there have been I don't know how many different attempts in the past (though not in medicine) to define social change. I cannot attempt to mention them all, but one which may have a little relevance to the way our discussion was going is that of Arnold Toynbee. He divides, in a very definite manner, countries and peoples into those which have, and those which have not, a 'history.' By this he means that there are countries or peoples whose lives go round in circles. They may have petty wars, etc., but essentially they return afterwards to the same point that they started from and hence their story is all repetition with change hardly entering in. In contrast to these, he singles out the major cultures or major civilizations as having history and hence as experiencing change. When they go through a war they do not return to where they started, but rather take a further step on what he claims to be the long cycle of development and decay. We might similarly view social happenings as change, or as not change, according to whether they seem to lead on in some process or merely permit the subjects eventually to return to their point of departure.

Another way in which we might look at it is by distinguishing between changes which the past history or past teaching of a people have given some idea of how to solve, and changes for which the past offers little or no guidance regarding solution. I would think that it is this group which most suitably falls into the category of real or serious social change from our point of view.

The question of defining social change according to the purpose that one has in view requires a whole paper. In my paper I have mixed up many levels, which has been justifiably criticized (Point

1). At times the paper operates on the personal level but most of the time on the community or population level, and these admittedly need to be distinguished in a way that I just did not trouble to do. Just how we can move over from one level into the other, and just how we can relate one category to the other is an interesting problem which needs study. We should, in other words, be looking into the question of how far clinical data can be used for answering problems which arise from epidemiological material and vice versa. However, I don't agree at all with the question of the division between epidemiological data and etiological data (Point 7.). I think data are data. The difference is not one of types of data but of types of hypotheses or types of logic. It is really the classic difference between inductive logic and deductive logic.

Epidemiological data are observed facts, and as such can be used to test hypotheses in the ordinary fashion. One forms an hypothesis and then asks oneself—Under what sort of situation would I expect, from this hypothesis, certain differences or certain patterns to appear. So one hunts through the available types of data to see if the type of situation which one requires for the testing can be found and if data on it are available. That is the way in which I believe epidemiological data can be best used for testing, and in terms of deductive logic I think such tests do offer proof insofar as proof is ever possible at all. (For, of course, there is a school of science which believes that absolute proof is never possible. All that is possible in this theory is the attaining of a further step in an evolving knowledge, a step which will be corrected in turn, at the next level that the body of knowledge reaches.)

On the question of method, which a number of people have referred to (Points 3, 4, 5, 6), I may say that this has given me considerable concern recently, and that I am at the moment designing a study to try and meet many of the points which have been raised. It might be of a little interest if I sketched it briefly, since it indicates how I think the problem should be tackled in one particular sector, immigration.

What I want to try and do is compare a random sample of prospective, accepted emigrants from a country with a matched sample of people who have not declared any intention to emigrate; and then with the same instruments compare these with population samples in a country of resettlement, or a series of countries or resettlement.

The latter samples would in part be natives, in part immigrants from the homeland originally studied, and in part from some other homeland. The core of the study would be material collected through directed interviews of the various samples, but this would be augmented by standard epidemiological data on mental hospitalization, suicide, delinquency and crime for the various populations being sampled, and there would also be a psychiatric interviewing of smaller samples. This is the way in which I see the problem which Dr. Carstairs and others have posed, and the way in which I see it as being answered; however, I have no money for the study yet.

I think a specific question was asked on the source for my statement about schizophrenia being reduced in wartime Norway (Point 8.) This is Ødegaard's paper on wartime incidence<sup>14</sup> and it refers to hospitalization, of course, not to so-called true incidence. His analysis of the estimated duration of the disease prior to admission in this same group suggests that there was no increased delay and hence that there was a true decline in schizophrenia in the community. It is debatable why it occurred and there is no evidence on the subject, so that it is purely my own hypothesis that I offered in my paper.

I don't think there were any other questions specifically addressed to me, and so I think that is all I have to say.

<sup>14</sup> Reference (65).

## CULTURES AS A CAUSATIVE OF MENTAL DISORDER\*

ALEXANDER H. LEIGHTON, M.D. AND JANE M. HUGHES, PH.D.

### INTRODUCTION

A REVIEW of the previous papers makes it evident that mental disorder is considered to be the product of multiple factors. The present paper is in harmony with this orientation, and its title, which was assigned to us, should not be interpreted as implying ideas of mono-causal relationship.

The discussion of our topic will be necessarily limited and selective, since talking about culture in its global sense touches on virtually all aspects of human behavior. Some areas such as family relationships and social change have been discussed earlier. Others such as cultural history and philosophy are too vast to be treated adequately in one chapter. We shall attempt, therefore, to present some points from salient literature, and to give impressions derived from several years of research dealing with socio-cultural factors and mental disorder.

### DEFINITION OF CONCEPTS

*Culture.* As used here "culture" is a label for an abstraction that encompasses the total way of life of a group of human beings.

Many other definitions have been proposed, and several variants are current in the social sciences (25). Leslie White, for example, employs the word to mean a pattern of history which can be analyzed and understood without reference to the human beings in whom it is expressed (46). Culture in this sense is a determinant force which follows its own laws irrespective of individual psychology and acts upon, rather than interacts with, human personalities. Such a conceptualization provides a way of explaining other phenomena by means of culture as the causal element. We think, however, that despite some possible usefulness in White's "culturology" with regard to understanding the evolutionary path of society as a whole, it is too

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divorced from human variation to have relevance for the malformations and malfunctionings of personality known as mental disorders.

Other ways of defining culture point to the material artifacts produced by certain societies and to the relationship between patterns of livelihood and environmental resources. Our concept includes all these factors—history, adaptation to physical environment, technology—but its focal point is what Hallowell has termed the “psychological reality” of culture (15). By this emphasis, culture refers primarily to the shared patterns of belief, feeling, and adaptation which people carry in their minds as guides for conduct and the definition of reality. Besides concerning all aspects of human life—social relationships, economics, and religion, for example—culture as a totality contains patterns of interconnections and interdependencies.

Although all societies have a cultural heritage which is transmitted from one generation to the next, the particular style varies from one group to another. Where contrast is marked, it is possible to speak of different cultures. Thus cultures have been grouped as “Western and non-Western,” “hunting and gathering,” “agricultural,” and “industrial” (17), or as “peasant societies” and “great traditions” (39).

In studying cultural factors which affect mental disorder, modern urbanites are, of course, as much the focus of attention as non-literate tribal groups. It is a common practice, however, to direct analysis toward situations which offer contrast to what prevails in our own culture with the hope of moving thereby into greater understanding of problems to which we are somewhat blinded by their being too close to us. It is for this reason that the examples to be cited here deal mainly with non-Western cultures, and the literature reviewed is primarily from the field of anthropology and the subfield “culture and personality” in which anthropologists and psychiatrists have collaborated.

*Mental Disorder.* Coming as it does at the end of the symposium, our definition of mental disorder should need little

elaboration. It is in keeping with the symposium's inclusion of all those behaviors, emotions, attitudes, and beliefs usually regarded as in the field of psychiatry. Such breadth of definition means that neuroses are encompassed as well as psychoses, sociopathic disorders as well as psychophysiological disturbances. It also means the inclusion of brain syndromes and mental retardation—conditions not primarily based on psychological experience but subject nonetheless to the influences of culture through practices of breeding, diet, care of the ill, use of drugs and intoxicants, and the training of the defective child.

#### HOW CULTURAL FACTORS MAY BE THOUGHT TO AFFECT PSYCHIATRIC DISORDER

As a means of organizing pertinent ideas, what follows will be presented as a series of statements, each one supplying a different way of completing the sentence "Culture may be thought to. . ."

1. *Culture May Be Thought to Determine the Pattern of Certain Specific Mental Disorders.* Names representing culture-specific disorders are well known in anthropological literature although they are not part of the standard nomenclatures of Western psychiatry. A list would include "amok" and "lâtah" both found in Malay (2, 43, 48), "imu" among the Ainu of Japan (47), "koro" in China (44), "witiko" among the Ojibwa Indians of the Northeast Woodlands (27), "piblokto" in the eastern Arctic (3), and "arctic hysteria" in Siberia (20). Each one embodies a constellation of symptoms found primarily in a given culture area, and often there is association between cultural beliefs or practices and the content of the symptoms.

"Witiko," for example, takes the form of a homicidal spree during which the individual may kill and eat members of his own family (7). In what could be called a delusional excitement the patient believes himself possessed by a spirit from his cultural mythology, the Witiko, a hoary cannibalistic monster



with a heart of ice. "Koro" is an anxiety state in which delusions concern withdrawal of the male sexual organs into the abdomen. It is associated with fear of death in a culture where it is believed that the sexual organs do disappear from corpses. Among the Eskimos, "piblokto" refers to a temporary derangement during which various bizarre acts are carried out such as dashing out naked into subzero weather or mimicking the sounds of Arctic birds and animals.

"Lâtah," "imu" and "arctic hysteria" are characterized by involuntary imitating, automatic obedience, shuddering, and fright. It is believed that women are more frequently sufferers from this disability than men. In some cultures certain people, especially old women, are known for this affliction, and it is considered sport to use gestures or words which will set off a reaction in which the victim goes into unseemly postures, dances to exhaustion, disrobes, and even harms herself or others.

There are accounts of whole groups of individuals becoming afflicted with a kind of mass hysteria, recalling the "dancing crazes" in Europe during the Middle Ages. One report tells of an instance in which a Cossack officer was drilling a group of Siberian natives. Each order he issued was shouted back first by one individual and then gradually by a chorus of all in the ranks. Every man appeared trapped in an exhausting and self-defeating repetition of the orders (and then curses) uttered by the increasingly infuriated officer (8).

A number of explanations have been invoked to account for such disorders. These comprise the ideas that they are:

1. Reactions based on physical disease such as malaria, tuberculosis, or luetic infection, but patterned in expression by cultural elements (43).
2. Reactions to the stress of severe environment, starvation, or long periods of isolation (37).
3. Reactions to the stress and strain of role characteristics in the culture (1).
4. "Hysteria" (6), that is, variations of a syndrome familiar in Western clinics and which is referred to in the American

Psychiatric Association nomenclature as "dissociative reaction" (4).

These explanations are not mutually exclusive. Some of the culturally localized syndromes can be considered as neurotic states involving suggestibility, and in which the content of symptoms is produced by the experience of growing up in a particular culture and being inculcated with its shared sentiments. Contributing factors may then be the stress of environment or roles. Dynamic mechanisms or noxious agents can also be regarded as components in the origin and course of the disorder.

The idea that these disorders are hysterical should, however, be treated with some caution. This is said partly from our feeling that such a conclusion is deceptively complete and hence may cut off effort toward penetrating to a less superficial level of understanding. There is also the possibility that it expresses a bias of the Western clinician who may have some tendency to consider any seemingly bizarre behavior as hysterical if there is no organic basis and if it cannot be called schizophrenia. This is further encouraged if the person exhibiting such behavior is uneducated from the Western point of view, is "simple" and "child-like"—qualities which are part of the stereotype we hold of "primitives." It would seem wise not to blanket aberrant behaviors found among the people of this or that culture with the term and concepts of "hysteria" (or of schizophrenia for that matter), but rather examine to see if some cases, at least, may not be on a somewhat different basis from what we are accustomed to see in the West. And even when "hysteria" turns out to be a valid label such an approach might, through comparisons and contrasts, increase our knowledge regarding the nature of the condition, not only as it occurs among non-Western peoples, but also among ourselves.

*2. Culture May Be Thought to Produce Basic Personality Types, Some of Which Are Especially Vulnerable to Mental Disorder.* The concepts of "basic personality type" (21, 22,

33), "modal personality" (16, 19), and "national character" (35, 14) were developed by anthropologists and psychiatrists to account for the fact that certain personality traits and certain inclinations to symptoms of psychiatric significance seemed to be associated with growing up in particular cultures. Being middle class American, Japanese, Russian—or, as described in Ruth Benedict's classic volume, being Zuni, Kwakiutl, or Dobu (5)—appears to predispose individuals toward particular kinds of symptoms. In the employment of these concepts, culture and personality were held to be essentially two aspects of a single phenomenon (42). This opened the way for studying personality through cultural data rather than through the behavior of individuals. The early work in this field by Kardiner and Linton had its foundation in exploring ethnographies and the folklore of non-literate tribes. Through analysis of child-rearing practices, kinship arrangements, socio-structural stresses, and especially religion and myths considered as projections of common, underlying personality attributes, "basic personality types" were postulated for different cultures.

Basic personality was thought of as a central core of values and attitudes which culture stamps into each of its members—a common denominator underlying each person's individual elaboration of life experience. Once a type had been described, it could be assessed from the psychiatric point of view as to its vulnerabilities. Thus, if at the cultural level—that is, group practices and beliefs—patterns were found that had psychiatric implications it was assumed that individuals in that culture would have these as psychological weaknesses. Whole cultures were described with psychiatric terms heretofore reserved for diagnosing individuals. If a society exhibited patterns of suspiciousness, hostility, witchcraft fears, and ideas of grandeur as in the potlatching Indian groups of the Northwest coast, there was a tendency to call such cultures "paranoid."

Since a major component of almost every clinical definition of psychiatric disorder is some deviation from the expected behavior and shared sentiments of the group to which the indi-

vidual belongs, the use of clinical terms for conforming, group-oriented behavior involves a contradiction. At best, it is the employment of unclear descriptive labels to characterize patterns of behavior manifested by a society. At worst, the clinical implications of the words are transferred to the group behavior, and dynamic interpretations are made in this framework. Since the behavior of people in accord with and at variance with group patterns implies major differences of psychological process, these usages can be exceedingly misleading. To say that a group is "paranoid," for instance, may be passable though not admirable if by this is meant behavior that is suspicious and hostile. If however, the word is intended as some kind of explanation based on individual psychology, then many pre-judgments and unsound inferences from individual to group behavior may enter the picture. One runs the risk of anthropomorphizing the group and regarding it as a deviant individual among a number of other anthropomorphized groups. It is one thing to say that functioning at the personality level and functioning at the socio-cultural level display similarities, and that how well they fit together is significant for adequate functioning at each level. It is another thing, however, to go beyond this and use identical terms in referring to these different levels of abstraction. This is especially true when the psychiatric terms invoked to identify and classify cultural patterns are not well standardized even at their source—psychiatry.

Theories concerning basic personality may also be criticized for a tendency to consider cultural factors as over-riding variations based on genetic influences affecting temperament (13) and for ignoring the possible effects of endemic disease and other physiological factors. For the most part "basic personality types" have been derived solely from cultural behavior or from the results of projective tests like the Rorschach. Thus far vulnerability to, or resistance against, mental illness has been postulated without concomitant investigation of the actual distribution and patterning of psychiatric disorder in the population.

Our own inclination is toward a less specific functional view of socio-cultural groups and the personalities which compose them. By this is meant the aim of understanding how psychiatric disorder can arise, take shape, and endure, as a result of interaction between individual functioning (personality) and group functioning. Since a discussion of this viewpoint has been previously published by one of us (30), we shall not here elaborate it further.

3. *Culture May Be Thought to Produce Psychiatric Disorders through Certain Child-Rearing Practices.* This point is closely allied to its predecessor. The difference is that while basic personality types have been formulated from looking at cultures as wholes, the focus here is directly and more exclusively levelled at socialization practices and the early years of life experience. Freudian theory has provided a means of organizing data from different cultures with regard to toilet training, nurturing, control of aggression, weaning, and encouraging independence (11). It has also provided a way of interpreting cultural variations with regard to probable significance for mental disorder among adults. Cultures portray remarkable variation in customs such as swaddling, use of a cradle-board, bottle or breast feeding, varying modes of punishment and reward, and permissive or restraining parental attitudes. This has given impetus to many hypotheses regarding the differential occurrence of psychiatric disorders.

The risk of this approach is to give undue emphasis to one set of factors, and to one period on the life-arc of individuals, to the exclusion of all other factors and periods of personality growth and development. Few would quarrel with the importance of the early years of life, but to assume that the experiences of infancy determine everything that comes afterward so far as origin, course, and outcome of psychiatric disorder is concerned, is to assume more than the knowledge currently at our disposal warrants. Different sets of dynamics are relevant to individual functioning at different stages of life. Physiologi-

cal and psychological changes in maturation and involution are probably of considerable significance in some kinds of mental disorders. Since our interest is in discovering cultural factors relevant to the whole range of psychiatric illnesses, it is important to recognize that adolescence, maturity, and senescence are viewed and defined as variously in different cultures as is child-rearing.

4. *Culture May Be Thought to Affect Psychiatric Disorders through Types of Sanction.* It has long been accepted that there is a relationship between some kinds of disorder and the manner in which a patient handles the problem of conformity or non-conformity—the sense of being right or wrong in the eyes of his social audience. There is considerable variation among cultures regarding how punishment is meted out to those who defy accepted beliefs and standards about what ought and ought not to be done. Cultures also vary in what is defined as transgression and the kinds of responsibility demanded of members. Some groups operate on the principle that society at large is the controller of moral conduct; others appear to maintain social control by implanting in individuals the job of self-monitoring conduct. These two types—"other-directed" and "inner-directed" in Reisman's terminology (40)—have usually been called "shame" and "guilt" cultures in anthropological literature. A critical discussion of this orientation is given by Piers and Singer (38). It has been thought that distinctive forms of psychopathology may be found in "shame" cultures where the atonement for sin calls for some kind of public demonstration such as a confession, while other kinds of symptomatology may be fostered in "guilt" cultures where expiation is left to the lonely world of conscience. One can theorize that where the group as a whole is the court to which account must be made, there would be a tendency for psychiatric disorder to take the form of antisocial behavior, aggression of the sociopathic type. Where individual super-ego is stressed, there might be an inclination to self-directed punishment and depression. In



short, and in overly simple terms, one type of culture can be thought to encourage symptoms which are disturbing to the group, while the other encourages symptoms which are disturbing to the individual who has them.

With regard to the kinds of behavior for which people are punished, it has been noted that some cultures institute negative sanctions only against what is defined as controllable, while others include involuntary behavior as well (23). Among some peoples, menstruation, multiple births or impotence are thought to be defiling to the whole group or at least an affront to cultural expectations. In a personal communication Dr. T. A. Baasher of Khartoum North has told one of us\* of the Ingassuma tribe in the Sudan where it is believed that the mother of twins has the evil eye. He reported an instance in which such a mother committed suicide by running her head-against a rock while the members of her village looked on.

The psychological burden related to the occurrence of certain uncontrollable and not uncommon events, and to some kinds of physiological processes, e.g. menstruation, may be of a magnitude that makes it appropriate to say that a given culture has a serious potential for psychiatric disorder. At least it seems clear that sanctions of this nature have a quite different meaning with regard to mental health from those which relate the occurrence of insanity to more or less self-willed acts such as breaking incest taboos among the Navaho (41), or masturbation as found in some of the folk beliefs of our own culture.

5. *Culture May Be Thought to Perpetuate Psychiatric Mal-functioning by Rewarding It in Certain Prestigious Roles.* Under the last point attention was focussed only on negative sanctions. We turn now to the positive side—reward—and also more explicitly to the concept of role (32). The relationship between socio-cultural role and mental disorder is complex, and we shall deal with it in two parts: here in terms of roles

\* AHL

which may attract individuals who have certain disorder tendencies and in Statement 6, below, in terms of roles which may produce some types of psychiatric disorder through being seats of conflict and stress.

In non-Western cultures the roles of medicine-man and holy-man—shaman or sahu—are examples of social positions for which, it has been thought, personnel are recruited from unstable members of the culture—hysterics and psychotics (24, 9). Taking the shaman as an instance, behaviors connected with the role have been described as indicative of disorder because emotional lability and frenzy characterize the seance, because the shaman has charismatic dominance over the group of individuals for whom the curing ceremony is held, because the shaman believes that he loses his own identity and becomes possessed by an over-world spirit, and because a fit or epileptic-like seizure culminates the performance.

There are, however, some considerations to be taken into account in following this line of thought. Just because the shaman's behavior resembles psychiatric symptoms is not a warrant for assuming that they are in fact psychiatric symptoms. Whatever else it may be, his behavior is part of the role of shaman and hence it may or may not have a relationship to his personality as a whole which would qualify him as mentally ill in Western terms. The settling of this question would require a thorough psychiatric examination of the person. To make a clinical diagnosis on the basis of role behavior alone is scarcely on a firmer basis than making a diagnosis from cultural patterns as noted on page 46.

What in shamanistic behavior may appear hysterical or psychotic to the Western psychiatrist is, to the people concerned, a time-honored ritual through which practitioners heal sick people or divine the future. Hence the "symptoms" of the shaman may in fact be the result of learning and practice. His role embodies a traditional plan for serving particular ends, and it is available in the culture as a model. The patterning of

behavior after this model can, of course, vary greatly in its success, from excellent to poor.

It can also be assumed that a variety of personality types will be attracted to the model and role for a variety of reasons, some making a conscious selection while others act in response to both unconscious factors and extraneous circumstances. In the cultures where shamans are found, there is usually much less diversification of roles than is the case in Europe and America. There the business of life may be managed through nearly all the men being hunters, farmers or warriors, with the women in the main being home-makers. The role of shaman, consequently, may be almost the only variant possible and it is thus likely to collect incumbents for a wide variety of reasons, some of a psychiatric nature, some for matters of temperament, some related to superior and creative qualities, and some based on physical abnormality—blindness or loss of a limb—which makes achievement of the more prevailing roles impossible. It seems to us, that while some shamans or medicine-men may be suffering from psychiatric disorder, this is probably not by any means the case with all.

The concept of role is traceable in part to 'role' as it is known in the theater. This may serve as a reminder that any given role as performed by an actor is not necessarily a direct and simple reflection of his own personality. Very few Ophelias have really been mad, and mad actresses do not necessarily perform Ophelia well. At the same time we do not wish to suggest that, because they may learn their part, most shamans are conscious fakers. On the contrary, it would seem likely that the ability to perform is enhanced by belief in the importance of the part.

In our own culture there are doubtless certain roles which resemble that of shaman in that they not only offer opportunity to mentally healthy personalities but also provide shelter for those with a certain amount of deviance. The artist comes to mind in this connection. Of course, many artists are mentally healthy, but it is possible for the arts to provide an opportunity

for an ill person to express himself creatively and thus have a position in the social system. Artists are often accorded leeway—indeed, may acquire prestige—in the expression of psychiatric symptoms which, if evinced by people in other social roles, might be reason for sanctions, or even hospitalization. Places such as the Left Bank, Greenwich Village, and North Beach give a social medium where fairly large numbers of sick people can float. These areas contain not only the genuine artist but shelter many who act like poets and painters without ever becoming highly original or productive. Certain religious groups and colonies have similar sheltering characteristics for malfunctioning personalities.

6. *Culture May Be Thought to Produce Psychiatric Disorders through Certain Stressful Roles.* With this statement attention shifts to the effects of roles rather than their patterning and appearance. It is possible to conduct analysis so as to identify roles considered to be psychologically damaging, even to the extent of producing psychiatric disorder. For the most part this approach has been typical of sociology, in contrast to anthropology's focus on child-rearing.

Roles can be considered stressful in a number of ways. One is the problem of ambiguous definition regarding expected behavior. This is especially true of new roles developed in situations of socio-cultural change where tradition gives no guidelines for assisting the recently emancipated to adapt and fulfill his new state. The principle is pertinent whether we observe a freed slave, a modern career woman, or a person in the limbo between magical and rational thought.

Roles may also present the person with inherently conflicting standards of behavior; the man who dedicates his life to humanitarian goals may come to feel he can reach a position effective for launching such a program only by being ruthless and competitive. Or a person may have to fill at one time several roles which make contradictory demands on his personality. We see this for example in students who have cast

themselves in the role of liberals yet attempt to be loyal offspring to conservative parents.

The relationship between role stresses and a particular kind of psychiatric disorder has been reported by Linton as occurring among the Tanala of Madagascar (34). These people have a condition called "tromba" which occurs mainly among second sons and childless wives. This is to be understood in the context of a culture in which inheritance and privilege are based on primogeniture and in which marriages are polygamous with the value of women related chiefly to child-bearing. Not only are role stresses and lack of social value involved, but also the mental illness itself gives opportunity for compensating prestige ("secondary gain"). Normally the family gives little attention to people filling such subservient roles as younger sons and wives without children, but for this illness the family group will finance an elaborate curing rite with attention focussed on the tromba-sufferer.

Innumerable other examples could be given of role stresses peculiar to this or that culture, and it seems probable that many of them are associated with some kind of psychiatric disturbance. It is a hard matter to pin down, however, for while individually persuasive cases can be found, research encounters problems of definition and the assembling of statistics adequate for conclusive statements.

7. *Culture May Be Thought to Produce Psychiatric Disturbance through Processes of Change.* It was intimated in the last section that some of the most striking examples of stressful roles pertain to cultural change—that is to say a given role is conflict-laden because of changes in the web of socio-cultural situations with which it is related. Being a wife and mother may take on this character if, in the changing cultural situation, a woman is also expected to hold a job, vote, be educated, and so forth.

Literature on the relationship between mental disorder and social change through immigration, mobility connected with

war, acculturation, and detribalization was reviewed in the last paper. It is not, therefore, appropriate to develop it further here except to indicate that culture is not static social organization and that in the world today, any study of culture is of necessity a study of change—changes of various sorts, at various rates, and with varying degrees of integration and conflict. Although there are numerous methodological problems connected with the use of hospital admission rates or projective tests, we feel that with advances in methods of case finding it is in the area of cultural change that some of the most revealing findings will occur that bear on the relationship between culture and mental disorder (31).

8. *Culture May Be Thought to Affect Psychiatric Disorder through the Indoctrination of Its Members with Particular Kinds of Sentiments.* There is now considerable literature in the social sciences on the differences between cultural groups in regard to socially shared feelings and ideas about man, nature, and reality (18). For the most part this has been concerned with values or beliefs held by relatively normal individuals. Implications regarding psychiatric disorders have, however, been pointed up in a number of ways. It seems probable that some cultures equip people with patterns of fear, jealousy, or unrealistic aspiration, which may foster mental illness; other cultures may be based on themes of self-acceptance and a relationship to natural forces which are more conducive to mental health.

Reality-testing in the tradition of Western empiricism is, for instance, a criterion advanced by modern psychiatry as an essential component of sanity and mental health. With such a base for discrimination, it has been suggested by Kroeber that the practice of magic and witchcraft and the adherence to non-objective beliefs characteristic of "primitive" peoples indicates a diffuse and subtle paranoia (24). Few would argue against the value of reality-orientation as a mark of psychiatric health, but, as many have pointed out, the standard cannot be



determined exclusively by scientific rationalism. A better criterion is whether or not a person is capable of assessing and acting in response to reality as it is defined by the group in which he grows up. This opens the way for understanding the relationship of religious faith, folk belief, and emotional coloring of attitudes to the development and maintenance of healthy adjustments and maladjustments. From such a perspective have come attempts to employ concepts which emphasize equally the cognitive, affective, and basic-urge (largely instinctual) forces which come into play in human functioning, and in that light to analyze the significance of differences in the cultural patterning of belief. The Eaton and Weil study of mental illness among the religious communities of Hutterites takes this aspect as one of its points for analysis (10). And it is central in the Stirling County Study (30).

9. *Culture per se May Be Thought to Produce Psychiatric Disorder.* All human beings are born and develop in cultural contexts which impose regulation of basic human urges. It has been thought that this is both universal and psychologically noxious with repercussions evident throughout the human race. We may all be, in short, like Chinese women with bound feet. Variations, however, are to be found in the degree of impulse-repression. Thus according to this view, simple and "primitive" societies with cultures which permit expression of sex and aggression are, on the whole, a healthier environment than complex, modern civilizations which compress infants into highly artificial patterns of existence. This is the kind of thing Freud had in mind when he spoke of 'civilization and its discontents.' (12)

Most social scientists today would not accept such inherent assumptions about the character of "primitive" and "civilized" cultures. The distinction has limited usefulness and then only when the terms are carefully defined. The more we have learned about "primitive" cultures, the more impressed we are with their potential for being both repressive and suppressive.

There is much in favor of the general idea that some kinds and degree of psychiatric disorder may be the price paid for being socialized, somewhat as backache and curvature of the spine may be part of the price paid for walking on our hind legs.

10. *Culture May Be Thought to Affect the Distribution of Psychiatric Disorders through Patterns of Breeding.* This statement and its successor—the final point we shall present as a way in which culture may be thought to relate to mental illness—stand on a different basis from all the previous items. Until now each statement has shared with others the characteristic of assuming that psychological transactions are the main, if not the only intermediary between cultural factors and the emergence and shaping of psychiatric disorder. This has, in fact, been the principal orientation of those concerned with culture and its bearing on mental disorder.

Culturally-prescribed inbreeding is found in many groups of people, particularly with reference to some non-Western cultures, elite families, and small communities which for one reason or another live in isolation. If such groups begin with a prevalence of hereditary factors which make for mental retardation, schizophrenia, manic-depressive psychosis or other forms of emotional instability, it is to be expected that these conditions will become accentuated and prevalent in the group. Laubscher's early work in the field of cross-cultural psychiatry illustrates an attempt to relate the amount of schizophrenia among the Bantu of Africa to the pattern of cross-cousin marriage (29).

The same kinds of factors may be at work at more subtle levels, and in larger groups. Thus the accumulating evidence in the West that there is greater prevalence of psychiatric disorder in the lower socio-economic ranges, has one explanation in terms of a socio-cultural process which produces a downward drift and interbreeding of people with genetically determined disabilities.

Heredity as a factor in psychiatric disorder suffers both from

over-emphasis and neglect. Heredity as such is considered *the* matter of importance in many centers of psychiatry, particularly in Europe. But the question of cultural patterns and their shaping of hereditary processes is scarcely considered, at least in any systematic way. In other psychiatric centers—especially in the United States—and among most social scientists, the whole of heredity is by-passed in favor of psychological factors. Here culture is apt to be given more emphasis but not in connection with the distribution of genes.

11. *Culture May Be Thought to Affect the Distribution of Psychiatric Disorder through Patterns which Result in Poor Physical Hygiene.* Our concern here is the role of physiological factors as the intermediary between culture and psychiatric disorder. Culture and cultural variation can be supposed to influence the distribution of noxious agents and traumata, and also the distribution of compensating factors and capabilities for resistance. In many non-Western cultures, for instance, contacts with the West which have demanded acculturation and abrupt industrialization have been accompanied by the spread of syphilis, tuberculosis, and many other diseases. Directly and indirectly these can foster disorder, although some have more potential in this regard than others. Of equal importance to the introduction of disease through contact, is the lack of native preventive and therapeutic measures.

Diet, based not only on availability of resources but also cultural preferences, may result in vitamin deficiency and malnutrition which in turn can affect the nervous system. There may also be cultural practices about child delivery, or the use of herbs and concoctions which make for brain damage. In some areas drugs have widespread use in native therapy, in recreation, and in religious ceremonies. There may thus be long-term degenerative effects as well as more immediate toxicities.

#### CONCLUDING NOTES

Given the impressions sketched above, what conclusions can

be drawn with regard to epidemiological studies of psychiatric disorder in different cultures as a means of expanding knowledge of etiology?

One can say to begin with that if the emphasis is on a primary target of inquiry such as genes, damage to the brain, or family relationships, the cultural context will be of some importance even if secondary. It will be one of the sets of factors to be considered in understanding how the damage comes about—whether *via* hereditary, physiological or psychological means—how it is spread and perpetuated and how it may be controlled.

If we take culture-in-relation-to-psychiatric-disorder as the primary matter for attention, then a major gap is apparent: an incomplete descriptive account of the varieties of psychiatric disorder to which human beings are susceptible across the world. The magnitude of this gap becomes apparent as soon as one begins to look into it. We do not even have a reasonably complete account of psychiatric disorders as these occur in a selection of contrasting cultures. Many of the localized types of illness such as those mentioned on page 448 are actually based on very few observations, some of them carried out years ago by non-psychiatrists. Despite the fact that psychiatric clinics exist in many non-Western societies, problems of nomenclature, variable criteria, and a Procrustean emphasis on Western systems of classification make assessment and comparison very difficult. Beyond this is a void consisting in the unknown numbers of persons who, though disturbed, do not ever come to clinical attention.

The importance of supplying this lack in our knowledge bears first of all on the descriptive aspect of scientific procedure. While we recognize that not everyone would accept systematic description as a basic component of the scientific process, it would be a digression to argue the case in general terms here. Suffice it to say, then, that if one does believe as a principle that this has its place and contribution to make in the study of man (no less so than in the study of other creatures, or of

the earth's crust, or of the stars) then the gap is in obvious need of filling. Although it will take years of painstaking work by many observers, it is a necessary foundation on which to base other kinds of study.

Stepping down, however, from the level of general scientific desirability with its implied faith in serendipity, it is possible to point out a number of more specific goals and opportunities. For one thing, description paves the way for assessment of frequency—be this in terms of prevalence or incidence. Such counts will be essential ultimately, both in critical problems of basic research into etiology and in providing information for programs concerned with treatment and prevention.

Description and the use of these descriptions as criteria for counts of frequency (epidemiology), bring with them the need for developing a system of classification that will stand up across cultures. While this may look on the surface like a rather dry and laborious exercise in taxonomy, shafts run out from it into the foundations of psychiatry, and there may be consequences that will profoundly alter many accepted ideas and change significantly the way the field is perceived.

Psychiatry itself, like most of the rest of medicine, is a product of Western culture. As such, it embodies ideas of illness and wellness, of normal and abnormal, of well-functioning and malfunctioning, of adaption and maladaptation which have their roots in our own shared sentiments regarding the character of reality, of what is desirable, and of what ought to be desired. While the range in these matters is considerable in the West itself, cultural studies make it clear that it is not so great as when the whole world is considered. In other words malfunction, one of the major components of a definition of psychiatric disorder, shifts its character from culture to culture.

This problem is not necessarily limited to differences of shared preference and shared belief as supplied by one culture in comparison to another. It may involve not only feeling and knowing but also the process of thinking. The studies of Merrens and his co-workers using psychological tests in the Bel-

gian Congo suggest that natives who have had a European kind of education think like Europeans, while those who do not, retain a framework quite different from the Aristotelian logic which is second nature to most Westerners (36, 28, 45).

The indications of such plasticity and difference should not lead one to hold that the range of psychological variation is limitless and that there are no transcultural consistencies. Even today there is good reason for believing that universals exist. While definition of malfunction and threshold of tolerance may vary from culture to culture, it is almost certain that mental retardation is known in all, as are also symptoms very like schizophrenia and depression. One of the opportunities in cross-cultural studies is to discover and more precisely specify universals and differentiate them from more localized disorders. Such a step would be a major advance in narrowing the field of possible etiological factors requiring investigation and would point to some as being more important than others.

A system of classification, together with its definitions and underlying concepts, which would stand up across cultures and take into account the variable and less variable factors, would probably result in some rearrangement and reorientation for psychiatry. At the least it would call for assessment of etiological theories against a broader background and it should bring to the fore the notion that the etiology of diagnosis in this or that cultural setting is a matter that has to be understood before there can be understanding of the etiology of disorder.

Psychiatric disorders are not, however, the only relevant area in need of taxonomic consideration. A problem of equal importance is the development of a system of classification for ordering the socio-cultural environment in a manner relevant to our interests in the effect of socio-cultural factors on the origin and pattern of psychiatric disorders. While some consideration has already been given to cross-cultural and transcultural classification of psychiatric illness, very little has been given to categorizing cultures and social groups from this point



of view. Yet without this there is severe limitation in generalization, in cross-comparison, and in the identification of salient socio-cultural factors.

While it is our opinion that the problems just mentioned are of first-order importance, it is not our intention to assert that they are the only questions worth tackling. Our inclination is rather to feel that the broad context needs to be kept in mind in any specific study and the limitations recognized which will prevail pending development of systematic knowledge in the wider areas. With this reservation, there is much to be said for pushing ahead with particular studies such as those concerned with relating culture, personality, and psychiatric disorder.

It may well be that the descriptive studies of psychiatric disorders in non-Western cultures could be combined and articulated with investigations of culture and personality. For instance a common syndrome in the Western Region of Nigeria is excitement (26). It apparently shows up in the clinics there with far greater frequency than it does in Europe or North America. It is also a component of disorders which have other features as well. One has the impression, moreover, that excitement at a somewhat lower level, though still high by Western standards, is a prominent aspect of many personalities. It also seems that the culture itself sets a positive value on states of frenzy under certain conditions. What are the relationships of these behaviors to each other? Are there also hereditary and physiological factors to be considered? Is there, for instance, any connection with what appears to be an unusual frequency of malignant hypertension? What is the part played by cultural change?

The promise in pursuing such questions is not at present in terms of revealing highly specific relationships such as was done by Pasteur in his work with micro-organisms, but rather in assembling evidence as a means of feeling out the more and less probable hypotheses for later, more crucial investigation. It is largely a matter of finding suitable targets and discovering

the right questions to ask of nature—questions which are answerable by the further procedures of science.

What has been observed above with regard to studies of culture, personality and psychiatric disorder, apply also to investigations of roles, child-socialization, and other questions of a similar type.

With all cultural studies, the possible contribution of hereditary and physiological factors should be given consideration. Their recognition is important, just as is the case with culture when the primary emphasis is on one of these other topics.

In concluding our paper, we should like to return again to a point mentioned earlier. This is our impression that comparative study of change is one of the most fruitful opportunities for uncovering the nature of socio-cultural factors in relation to psychiatric disorder. We regard descriptions and analyses of cultures at a given time as prerequisite to this, as fixing-points in terms of which to understand shifts. If, following a suggestion made earlier, we were to attempt to build a system for classifying cultures in such a manner as to have maximal relevance for mental health and mental illness, we would choose types of socio-cultural change as our starting point.

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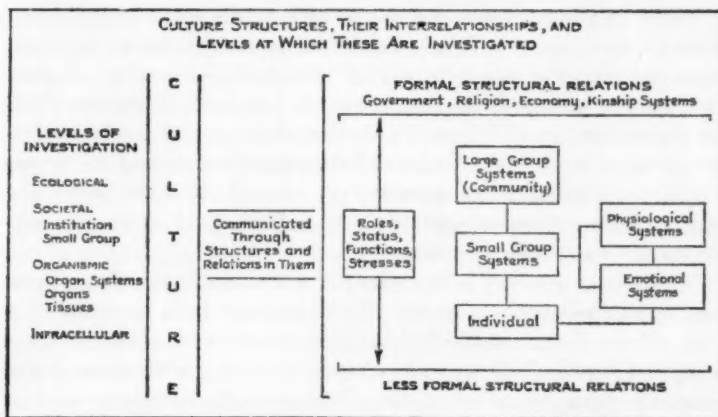
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# DISCUSSION

DR. GEORGE ROSEN: One of the things that has bothered me as we went along yesterday and today has been the fact that we have been discussing a very large, global subject without specifying the level at which, or the realm of discourse within which, the discussion has been taking place.

In certain areas this has been more explicit than in others. For instance, Dr. Böök made explicit what he was talking about when he started out, but some of the other discussions have not had this advantage. It might be useful if we were to look at the levels of investigation, at least as I envisage them, and I have made the following diagram or list which ranges all the way from the ecological (conceived very broadly here as relationship of organism to environment), through the societal (which is one aspect of the ecological) and the various structural elements of the society (institutions, the small group), and then the organismic level (the organ systems) and finally the intracellular aspects.

Dr. Leighton has given a definition of culture which is in some way related to any one of these levels. I think as I go along you will see that while I could follow this, I am going to stick to certain things simply because Dr. Leighton has made my task in one sense easier and in one sense more difficult. He and his coauthor have set forth a series of specific propositions. I can take issue with these propositions if I wish, and I will to a certain extent, or I can disregard



them, which I have done at certain points. I have a number of degrees of freedom in this respect.

Also in relation to culture, there is another element that I think one ought to keep in mind as we discuss this paper. Culture is one abstracted aspect of a global entity, namely, a society, and a society conceived as a system. In this connection, one has to keep in mind that when I talk about culture there is always implied the concept of structure, and that there are various avenues and channels through which culture is both produced and disseminated. In other words, one can think of religion, religious institutions, as producing what we call culture and at the same time being affected by what we call culture.

What are we dealing with? An analysis of culture as causative of mental disorders may usefully begin, as Dr. Leighton has done, by defining culture. It is a concept which sums up the patterned ways of acting, feeling, and thinking of a group of people; the ways in which they arrange and interpret group life, both cognitively and affectively; the symbols used for this purpose; and the levels of awareness on which these processes occur permeate individual and group behavior. In varying degree the members of the group are shaped by the culture psychologically and physiologically. Culture is not a matter of a single generation, but is shaped by the past history of the group. Here I would take issue with Toynbee's position that there are groups without history and groups with history. This is peculiarly Toynbeean.

I think in a larger sense one has to look at the social organization of the group to see that it is not only permeated by culture but produces culture. For example, one of the more interesting religious groups of the present is the sect known as Jehovah's Witnesses. This sect represents the 20th century version of a cultural tradition that can be traced back to the Judaeo-Christian culture around the beginning of the Christian era, namely, the idea of the Last Days, the coming of the millenium and the end of history; in short, the sect derives from an ancient eschatology.

Culture is in one way independent of individuals and affects them even when they are not aware of such action. It is in terms of a given culture that an individual learns to perceive his social environment, and here I think we can link our discussion with some of the things Dr. Murphy talked about. Consequently, culture as an his-



torical product and as an element in social processes must be seen along the axis of both past and present time. In other words, how a culture developed, what it meant in the past, and what it means in the social dynamics of the present, are all revelant to our topic.

Our topic may be viewed from this standpoint. Most of the data presented at this conference have been from the present or the recent past. I believe it may be useful, therefore, in treating or discussing the subject of culture as causative of mental disorder, to offer some data from the distant past as well as from the present.

I start out by taking Dr. Leighton's first point that culture may produce specific mental disorders, and I have chosen as an instance the famous religious revival in Kentucky in 1800 known as The Great Revival. For those who are interested in this, there are a number of sources. I might mention the relevant section in Frederick Davenport's *PRIMITIVE TRAITS IN RELIGIOUS REVIVALS* (2), a short account but a useful one. For a number of days thousands of people gathered to listen to preachers who exhorted them to repent, to cast off the devil, to be washed in the blood of the Lamb and to be reborn. Verbal pictures of fire and brimstone were created in sessions which went on for hours, often at night in the light of large bonfires. Soon a number of individuals began to fall down in a state of unconsciousness. Others rolled on the ground. Still others began to exhibit jerky, quivering movements, and yet others ran around on all fours barking like dogs.

This instance was not the first of its kind and not the last. Religious revivals have been a feature of the American scene and have appeared in other parts of the world. It is, however, instructive because it involves a number of factors which help us, I believe, to understand how a culture may produce such behavior as I have described. Most of the instances that I shall cite following this are cases of dissociative reaction or what has been termed hysteria.

First of all, it is essential to know the cultural context. This is the first methodological point I would like to stress: that in many of the reports that we have, there has been no real study of the cultural context.

The culture must be understood before one can undertake to interpret behavior as normal or pathological. For example, in the Kentucky revival that I mentioned briefly, the group had certain behavioral expectancies. Methodism had somewhat earlier produced

similar phenomena, and so had the earlier New England revival of the 18th century. Secondly, the concepts of Hell, the Devil, and other ideas were part of the culture and generally accepted. Thirdly, the circumstances: a large crowd, the darkness, and the shifting lights of bonfires created an atmosphere favorable to release of conscious controls and relief of tension. Finally, the need to release tension due to hardships and perils on the frontier is another factor that I believe cannot be overlooked.

Are there other comparable situations in which one can find similar factors? And do these factors operate in the same way? The Kentucky revival is a case from which one can derive certain factors for further study. Are there situations in which a culture intentionally elicits dissociative reactions?

Where the mystical experience is a desirable value, ways are developed to elicit such behavior. Heinrich Zimmer, in his discussion of Vedanta (13), says that the adept reaches a point in his spiritual development at which he becomes identified with the Personal Creator of the World Illusion:

He feels that he is at one with the Supreme Lord, partaking of His virtues of omniscience and omnipotence. This, however, is a dangerous phase for if he is to go on to Brahman, the goal, he must realize that his inflation is only a subtle form of self-delusion. The candidate must conquer it, press beyond it, so that the anonymity of sheer being, consciousness and bliss may break upon him as the transpersonal essence of his actual Self.

I will leave it to you to judge how similar this may be to certain psychopathological conditions. Analogous instructions can be found, for example, in the exercises of Ignatius of Loyola and in various other writings concerned with mysticism. Similarly, the Shakers had a method of rolling the head back and forth to elicit the ecstatic state which they desired. At some times they sang, and at others, danced. Here are very intensive definite patterns of behavior to elicit what, in the catalog of the American Psychiatric Association, is termed psychopathological phenomena or states.

Here I shall say a few words about the dancing mania to which reference has been made in a number of papers. This has been referred to in passing several times and illustrates the first point I made. Much of what is said about these phenomena has been taken

from the English translation of Hecker's *THE EPIDEMICS OF THE MIDDLE AGES* (5).

The German word for dancing mania is *Die Tanzwuth* which, correctly translated, should be the dance frenzy, *Wuth* meaning anger or, as in *rasende Wuth*, frenzy. There has been a semantic confusion here which tends to prejudge the issue of the occurrence of psychopathological phenomena. Secondly, closer examination of the context reveals that it occurred in a tradition of religious dances. There are still religious dances today, and there have been quite a number of studies of such dances up to the present in various parts of Europe within which the so-called dancing mania fits. Thirdly, the dance frenzy has elements of the kind found in the revival mentioned above. Fourthly, one cannot overlook the possible presence in such groups of choreics and of frauds, and I think one has to keep in mind at all times that such dancing groups are not necessarily homogeneous. Finally, these were not "crazes" or "manias." Some individuals may have been psychotic. Parenthetically, I wish to emphasize that what I deal with and what I present are anecdotal materials, and I have no apologies for this at all. I think one can learn a great deal from these materials. Peter Cartwright, one of the Kentucky revivalists, points out in his memoirs that a number of individuals became psychotic in the course of the revivals. Not everybody became psychotic, and I think here again we have a point of interest that might be followed up: in such a situation, who breaks down—who, in a sense, is predisposed to psychosis and who is not? We come up against some of the questions that we have been dealing with before.

Then these dance frenzies are not unlike some aspects of millenarian movements which flourish best in times of extraordinary social ferment. The participants may exhibit behavior which becomes ecstatic to the point where observers describe it as mass hysteria.

For example, in the 17th century England quite a number of the extreme groups of the Puritan Party exhibited behavior of this kind. Such phenomena may be considered, and this is an hypothesis that I present quite firmly, as necessary social devices for generating the superhuman efforts needed to change current conditions. I think one has to look at some of these phenomena as not necessarily evil or pathological in and of themselves, but even as useful under certain conditions.

Here we come again to the point that I have raised before, the need, in dealing with such global terms, to specify precisely what we are talking about, under what conditions and at what point. In a sense this is not very different from Dr. Gruenberg's point about checking with some kind of mechanism that will enable one to indicate precisely the conditions under which one is operating.

For example, just to cite something that has nothing to do with epidemiology, but in which I think one can again get an inkling of what goes on, Yeats in his poem on the Easter Rising of 1916 says

A drunken vainglorious lout.  
 . . . . .  
 . . . has been changed in his turn,  
 Transformed utterly:  
 A terrible beauty is born. (12)

This feeling of utter change is one which I think is quite relevant, because it may lead under other conditions to the disillusionment that occurs after such superhuman efforts, and here too the culture is necessarily involved. An interesting instance of this is the book, *THE NEW CLASS* (3) written by Milovan Djilas, the Yugoslavian communist who was jailed for his opinions. His deflation, in a sense, is very obvious as one reads the book, and he is now on the downward side of this curve of elation and deflation.

Another point on the role of the culture in producing these phenomena is actual learning. Dr. Gruenberg has dealt with some of this in his article on "Socially Shared Psychopathology" (4), which I have found extremely useful. I would like to cite, however, another instance.

Charcot, in the presentation of his *grande hystérie* very definitely taught his subjects to produce the phenomena. As one reads the records it becomes obvious that here within a definite cultural context, namely, the clinic of the Paris School, he was training hysterics, if you will, so that they could perform beautifully. The same thing occurs, for example, in the famous case of Urbain Grandier in 17th century France, which Aldous Huxley has written up so well in a very entertaining volume. For those who have not read it, it is called, *THE DEVILS OF LOUDUN* (8). In this case, too, we have a situation in which a number of nuns in a convent were taught to

enter into possessive states. They were told, not directly, but indirectly and by various cues, what to do, and they did it. The Salem witch trials exhibit the same phenomena. In consequence I think one can state quite explicitly—subject, of course, to refinement of the proposition—that cultures *do* produce pathological phenomena and states. They will not produce them all in the same way. Nor am I sure whether they will produce all of them.

I have been talking here about dissociative reactions. This may not apply, let us say, to psychosis; and I think here we have to recognize another point, that global expectations, just as global concepts, do not always function well. One has to break them down. The expectations have to be specified much more precisely.

The second point concerns culture and personality types. Here I care only to remark that we are dealing with an attempt to develop personality types with ideal type concepts, and as all of us who have dealt with ideal types know, this tool suffers from a number of defects. An interesting recent discussion of this problem is to be found in Barbara Wootton's *SOCIAL SCIENCE AND SOCIAL PATHOLOGY* (11), where she treats the relationship between psychiatric theories and juvenile delinquency and other aspects of crime.

The third point deals with culture as making it possible for psychopathological individuals to enter into certain roles, and this merits a little bit more time. For example, in ancient history prophets and diviners were quite common. Whether these individuals were mentally ill or not is a moot question. However, in more recent periods there are records of such individuals who have actually in times of stress been able to occupy positions and exhibit behavior which certainly falls within our area of concern.

An interesting one occurred in England in the middle of the 17th century. This is the case of Solomon Eccles, who in plague-stricken London of 1665, walked about naked with a dish of fire and brimstone on his head, prophesying woe. He forecast a universal conflagration for the following year. While of course London was destroyed, the world did survive, and he vanished. Eccles was accepted because the society of which he was a part was no stranger to persons of this type—to prophets of woe with odd behavior. They had the Ranters, Fifth Monarchy men, and other groups.

However, coming much closer to our time we find the example of Antonio Conselheiro in Brazil. Antonio "The Counselor" estab-

lished a New Zion at Canudos, in the backwoods of the state of Bahia to which the credulous flocked. The disorders becoming too much for the state militia, the Brazilian Army was despatched, only to suffer a complete rout in 1896. A second army group, after a lengthy and bloody siege which saw all of the men, and most of the women and elder children of New Zion fight to the death, finally restored order to the backlands of Brazil. As one reads the description of Antonio, both in the medical literature as well as in the book by Euclides da Cunha (1) it is quite obvious that he was a man with a religious mania. Just what the exact clinical nature of his illness was it is not easy to say. But certainly given a situation in Brazil very much like the backwoods of the United States in 1800, given a population living under severe conditions, given a population that would like something better, a man who comes along and promises them all sorts of things is obviously in the position to occupy a place of leadership, plus the fact that there have been other cases of this kind in history, both in Brazil and elsewhere.

There is, of course, the famous case of Johann Bockelson of Leyden, who became the King of the Anabaptists at Münster in the 16th century, and eventually reached the stage at which he instituted in his kingdom all kinds of new social institutions, including polygamy. Bockelson tended to lose contact with his environment by rebuking, punishing, and stopping all kinds of criticism. Recently Dr. Gruenberg, in discussing this matter, raised the point, which I think is relevant here, as to the extent to which the culture impedes feedback from various aspects of the community to the person who is the leader. In this sense, if I interpret him correctly, it makes it impossible for the individual to test reality and may predispose him, or make it possible for a predisposed individual, to suffer some sort of psychopathological condition.

The matter of child-rearing practices I think has been stressed considerably. Orlansky (9) and more recently the articles by Pineau (10) on the work of Spitz, have indicated the holes and the gaps that exist in this literature. I believe the comments yesterday by Dr. Carstairs on Bowlby are sufficient at the moment.

As to the matter of stress in culture, this is another point at which one may discuss the role of culture. Here we enter the area where we descend from the level of the societal to the organismic and the organ systems. I shall emphasize a point that I have made elsewhere



and earlier, namely, the need for working back and forth between various levels. I don't think one can stay on the epidemiologic level alone or on the societal level alone or whatever other level alone. I think at various points there has to be a contact between these levels. How one establishes it becomes a matter of methodology, plus a matter of how one classifies cultures or societies of various kinds or various other systems that I am certain are necessary in this area.

I would like to cite to you one or two studies which bear on this point. One is a report by S. R. Hill which includes studies on the adrenal cortical stress in man (6). The investigators took two rowing teams at Harvard for 1953 and 1954 and studied them. Their metabolic responses, as well as their psychological responses were examined in various ways, both before and after a race. The 1953 crew, as did the 1954 crew, responded very variably before the race, both in a metabolic sense as well as in a psychological sense. During the race, the 1953 crew tended to become more uniform in terms of metabolic response. The 1954 crew did not. Interestingly enough, the 1953 crew won the important annual race while the 1954 crew lost the race.

I think there is an interesting hypothesis here as to a type of research which, in my opinion, has not been sufficiently explored. It might even be attempted on an epidemiologic basis if one had a large enough population. There are, of course, difficulties in this sort of thing, and I am willing to admit them.

The other point raised by Dr. Leighton—the matter of inbreeding and so on—has been touched on before by Dr. Böök in his paper. I think it might be said, however, that we can actually trace a neurologic condition which has certain psychotic components. Using Huntington's chorea, as an example, we can follow how the disease was brought over to Long Island where Huntington lived and where he found his first case, and how he traced the whole genealogy of the disease in the involved families. Also, one can see as one goes back into the history of this area how some of the choreics were occasionally mistaken for witches and were involved in witch trials, and how some of them got involved in other things that I have mentioned earlier, particularly the revivalistic situation.

Finally, I shall end on two notes. One relates to the point that Dr. Leighton raised about systems of classification. I have indicated, for example, certain general approaches. With regard to cultural

classifications where we have great difficulty, I feel that there may be a value in going back to the earlier Hobhouse classification of various peoples (7). Using material artifacts and combining them with an ideologic classification, we can establish material-ideologic areas. This system might be related to another system, which might begin with an ecumenical type of culture, namely, a culture spread over an extremely large area, say the Graeco-Roman culture, or the European-American culture. This is then broken down into what one might call a national and regional variant of this culture. For example, within the European-American, we have the United States, France, Germany, and a number of other countries which certainly differ within this general context. Then within the national, one can have a regional classification or level, and beneath that a local level. Of course, one can make any subdivisions of such a classification as are necessary to deal with the available data. I have attempted to use this approach in certain studies that I am doing at the present time, and while I am not sure how useful it is going to be, I would like to advance the suggestion for discussion.

The last point relates to the cultural bias of the investigator. This is a point which, while it was touched on, requires, I think, much more stress and emphasis. I would like to call your attention, for example, to the earlier studies on immigration and migrants, and to the fact that many of the investigators had very definite biases against the immigrants. I cite, for example, an argument for restriction after the First World War. It came when social scientists produced statistics on the caliber of American soldiers—information derived from psychological tests designed to measure the intellectual capacity or inherited ability of the young men—which seemed to prove that 46 per cent of the foreign-born soldiers had a very low grade of intelligence. This supported the basic restrictionist contention that the new “immigrant groups” were altogether (aside from the factor of literacy) inferior-minded. Again, a distinguished scientist in 1921 pointed out that the low mentality prevailing among most of the foreign-born led them into pauperism, crime, sex offenses, and dependency; that the different forms of crime already associated with each ethnic group in the country were fixed by heredity.

This kind of thing in subtler form still exists at the present time. I think it is amusing, and at the same time thought-provoking, that we may be doing the same thing unwittingly or wittingly: culture

does have an impact on how we diagnose, or where we recognize mental illness, and on that note I would like to stop.

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SUMMARY OF DISCUSSION

(1) The suggestion that reality testing could be used as a yardstick of mental disorder for cross cultural studies was questioned. Since most people carry around with them their own concept of reality, the adequacy and relevancy of this yardstick depended upon the particular situation being examined. While various objective measures—such as the efficiency of economic productivity—do exist, these are not particularly relevant to the investigation of the distribution of psychopathology. Where other more “relevant”

measures exist, these tend to be relevant only within the observer's framework and are not necessarily relevant to the situation actually at hand. In reply, it was pointed out that to appraise a person's reality-testing required a knowledge of his culture as a prerequisite. Thus the Moroccan Jewish immigrant to Israel who frequently refused hospitalization and who, if brought into the hospital refused to sleep in a bed, lying instead on the floor, was reacting to the belief that white sheets were shrouds and the hospital bed a death bed. However, when the beds were made up with green sheets, the problem was overcome, and the patient was grateful and happy to receive the medical care he needed. Episodes such as these offered special opportunities to study defects in "reality-testing" on the part of the health professions as well as to correct their erroneous belief that such defects lay with their patients.

If the observer is to pass judgment on the reality-testing of those observed, it is essential to establish the frames of reference of both the observer and of the individuals or groups under study; to get to know what people think, what they do, and why. The concept is essential, since not only can the frameworks be different, but certain elements can be totally lacking; that is, the framework of the group being studied may not encompass the whole range of the "reality" apparent to an outside observer. The attempt of a backwoods' group, the Lazzaretti, to found a "New Jerusalem" in Tuscany during the 1850's or 1860's was cited as an example. The fact that this group might have been behaving peculiarly within the framework of a well-read, well-traveled visitor of the period, would not necessarily mean that they were not acting rationally within their own framework. There were thus two elements—two frames of reference—that had to be brought together before judgment on reality-testing could be passed: "The other fellow's and mine."

(2) It cannot be assumed that all mental disorders are the same everywhere, or even the same in the same place. They might take on different symptom forms, as the dissociative reactions obviously do, or an illness might not even be recognized as a distinct entity until a certain point in time as, for example, pellagra in the 18th century. While such exotic conditions as *lâta* or *koro* or arctic hysteria could all be classified under one general term, it is important to know what they mean within their own specific cultural contexts. Thus, they might not be regarded as pathological but as quite normal behavior,

because everyone had it, or because everyone who had it was accepted by the community. And yet, from the Western point of view these were pathological disorders. Given separate categorization of mental disorders and cultural factors, there might appear to be a basis for comparison between cultures. But in actual experience mental disorders could not be categorized independently of cultural factors.

(3) Another point dealt with the distinction between normal and pathological psychology. The types of behavior that had been discussed in Point (2) above, however bizarre they might appear could, perhaps, confer advantages or be the realizations of some adaptive capacities of human beings in certain kinds of situations. Thus people who went through dissociative reactions often felt better afterwards. Mystics, for example, generally derived a sense of well-being from their special experiences.

Whirling dervishes, by employing perfectly standardized ritualistic methods for inducing a dissociated state (such as breathing rhythms), were able to pass daggers through cheek and tongue. Their climax was a mystical experience and release; the kind of conversion phenomenon discussed by Stanley Hall.

Dr. Rosen's description of the Great Revival in Kentucky was exactly paralleled by one which recounts Billy Graham's recent meetings in New York. Some participants in the discussion were sure this did not involve psychopathology though such events might be seeded with pathological individuals and frauds.

Histories of churches showed a pattern in the development of these phenomena. Beginning with various types of release mechanisms which appeared to arise out of the insecurities of the population (such as frontier life or very low socio-economic standing) these traits disappeared as the churches became older and more solidly established. This, for example, was the history of the Methodists in the Midwest.

Were such an event as the current (1959) steel strike to continue long enough, it would be interesting to speculate about what might arise to take up the tension. Perhaps religious revivals are alternatives to revolution. Whether revivals might be considered healthier than revolutions is a matter of value judgment.

(4) Do such episodes as the Dancing Manias simply compress cases in the long-run occurrence of psychosis? Do they add cases to the long-run total? Or do they merely precipitate cases earlier than

would otherwise have occurred. It was pointed out that while mortality rates in New York City rise during heat waves they are followed by drops below normal; the heat apparently causing the chronically ill to die a little earlier than they would have otherwise. This question applies not only to phenomena like the Dancing Manias but to any social or cultural factor. Such factors might either add to the actual over-all risk, or only shift the risk to an earlier time.

(5) There seems to be no clearly demonstrated instance of either a cultural or a social factor being known to be a predisposing factor in mental illness. This is true whether the category used was a broad one, such as social disorganization or social isolation, or a far more limited one. The absence of clear-cut evidence does not show that the hypothesis is incorrect but only that it has not been demonstrated even once.

Twin studies give good evidence for genetic influence; on brain damage, there is quite definite evidence. Such evidence helps the investigator by serving as a starting point and as a point to fall back on. The absence of such evidence in the social and cultural areas regarding effects on the prevalence of mental disorder is a serious lack. The ultimate finding may be that culture is only important in determining a peculiar mode of expression and is not in itself a basic factor. The effect of culture on mental disorder may be a precipitating cause. Perhaps culture increases the risk of a particular set of symptoms of disordered functioning in people who would have developed a different form in another culture. If so, the general level of all types of symptom groups taken together would not be affected by variations in the type of culture.

The kind of person who shows different patterns of unusual action at different times in his life was suggested as an example. Thus in Britain there were young men who, starting as enthusiastic Oxford Groupers in college, became Communists, moved through a socialist phase, and ended up flirting with ultra-nationalism of one kind or another. These people were reacting to the changing social and intellectual environment of the times, and while each phase could be described differently, they represented but a single group of people. Perhaps this same sort of variation in overt behavior will be found in people with mental disorders in different social or cultural environment.

The view that culture is only important as determining the par-



ticular mode of expression of mental disorder might seem depressive and nihilistic, but it would be a great help to have it tested objectively.

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DR. LEIGHTON: Let me take up a few of the scattered points that have been raised and then try to pull together a couple of synthesizing notions.

I agree with Dr. Rosen's point about the importance of the cultural context in making an assessment of psychiatric disorder, but I would underscore this as a technical matter, over and above its being a question of steeping in another culture. Steeping alone is not enough. During the war, I had occasion to observe "Old Japan Hands" in action as advisers to the intelligence services, and found that their impressions were often inferior to those based on naïve but systematic analysis of data.

Speaking of Japan reminds me of earlier reference at this meeting to the Dancing Manias. I should like to mention in passing that they occurred more recently in Japan than in Europe.

The point brought out in the previous discussion on migration—namely, who migrates—is worthy of more attention. In our studies in Nova Scotia we have by chance been dealing with a place that exports people. A prevailing view of its residents is that the smart ones get out and only the stupid, or those with personalities somewhat handicapped emotionally, stay. My impressions would lead me to doubt this. It would be interesting to reflect on what kind of a country the United States or Australia would be if this kind of generalization about migrants had universal validity. The Nova Scotian studies suggest that the reasons for staying behind are as various as the reasons for migrating, and they can be lined up as much on the side of assets of personality as on the side of liabilities.

In answer to the question raised in Point (5)—whether one can fall back on something solid methodologically: we do not yet have anything as useful as the twin approach in genetics, even allowing for its disadvantages. There have been, however, a number of worthy attempts such as predicting from the child-rearing practices what kind of personalities, and what kind of tendency to breakdown, will be found in the adult population. This approach, however, has always struck me as being a good deal like looking up

the answers in a book of arithmetic and then working out the problem afterwards. One usually starts the analysis of child-rearing with some impression of the prevailing personality characteristics of people in the culture under investigation.

There is a fairly extensive modern study going on now, as you probably know, under the guidance of Whiting, which is trying to analyze and compare data that has been collected from many cultures.\*

A question which deserves a whole meeting to itself concerns what it is we are going to count. See Point (2). Let me hazard a few sweeping generalizations.

One is that no existing form of diagnosis is usable for the purposes we have been discussing. This applies not only to hospital records, but to what would result if the investigator went out into the field and tried to make his own diagnosis on a sample. He couldn't use any of the systems of diagnosis that are currently employed in psychiatric clinics, private practice, or anywhere else. Some modifications would have to be worked out.

One has to get rid of the built-in etiological pre-conceptions that exist in most diagnostic acts. Where studies are concerned with exploring the etiological influences of cultural factors, the psychiatric phenomena for study have to be defined in terms of symptom patterns. The question of whether they are pathological or not should be set aside. In short, one has to study the distribution of selected types of human patterns, and only later ask what the functional effect and consequences of these are. The determination of pathology is the last thing to be done rather than the first.

Admittedly, however, the selection of which patterns one is going to follow in this kind of an epidemiological study is based on one's conception of what the character of psychiatric disorders is—on what is seen commonly in psychiatric clinics. This is a very complicated, although not quite hopeless, subject. The difficulties of cultural relativity also tend to look worse when one is thinking about them in the abstract than they do when one is trying to compare the people of two different cultures.

The crisis situations that Dr. Rosen was talking about can be viewed as an extension in time of the kind of thing Dr. Hughes and

\* Human Relations Area File, Yale University.

I tried to describe when we mentioned havens like North Beach, Greenwich Village and religious groups. There may be more of those at one period than at another.

Turning now to a more general level of discussion, it seems to me that there are two approaches to the problem of evidence regarding the influence of cultural factors. One is through successive approximations. I visualize here the comparing of two cultures and finding that some kinds of symptom patterns have a statistically significant difference in frequency. With successive stages of refinement in criteria and methods, the precise cultural factors associated with these differences could, with reasonable hope, be ferreted out.

The other approach is that stressed by Dr. Rosen in his discussion: One can pick socio-cultural situations which one can expect, on the basis of psychiatric theory, to be loaded with a tendency to produce psychiatric disorder. Religious revivals might be one such, but there are obviously many others that would bear investigation: broken homes and upward mobility in the socio-economic system are others. These could be examined to see whether or not there is in fact a higher association of psychiatric disorder, and then why some people, in spite of these adverse situations, do not develop symptoms, and so on. The acculturation process, social change, low socio-economic status, and much else is susceptible of this kind of treatment.

I think these two sorts of approaches will have to be related to each other as the field progresses. Perhaps the first is where you get ideas for target areas, and then the second is what you do in order to find out more about how processes move along through time. It seems to me this fits with Dr. Densen's idea, that there needs to be a relationship between the extensive study and the intensive study.

Let us look now at the problem of classifying cultures. See Point (2). This is a topic that I also feel would be worthy of a whole meeting; however there are some points that can be made briefly. We need, of course, a system. We need a way of classifying which is workable and objective in the sense that different observers would be able to draw the same conclusions when operating independently of each other.

But this is not enough. The system has to be related to the character of psychiatric disorder. The little we know about psychodynamic processes has to be taken into account, so that the system of

categories for cultures will have a probability of showing significant relationships to psychiatric disorder.

It would be easy to develop systems that would organize cultures into categories that had reasonably clean margins, but it is not easy to do this and carve nature at the joints which separate the factors significant in fostering psychiatric disorder from those that are not. It is Brownowski, I believe, who has pointed out in connection with the classification of species, that Linnaeus could have developed a clearer and more measurable system if he had classified flowers according to color or weight or size. The fact, however, that he hit upon pattern as the basis for classification meant that he carved nature at the joints, which makes possible the development of concepts and evidence regarding evolution. The simpler, more definite and measurable methods would not have had this value.

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DR. MACMAHON: Thank you very much, Dr. Leighton. I have been trying to get in a quotation ever since we were talking about core schizophrenia, and you have let me manage it very nicely. It is by John Stuart Mill. He is describing an entity, "It is a piece of barbarous lath invented by school teachers and taken over by the logicians to stop a leak in their terminology." That closes the scientific session, if that is the word for it.

DR. GRUENBERG: I just want to express my thanks to all of you for the hard work that has been done in preparing for the meeting. While I suppose I should take particular note of those who prepared the papers, I must also voice thanks to those who read them so very carefully before the meeting. I have never seen a meeting done this way before—where there were no formal presentations, and where the group which assembled had read a common body of material before the discussion. It was an experiment, and my own evaluation is that the experiment has been a success. That it is so is, I think, due to the conscientious work that all of you have put into it, for which I am very grateful, and I thank you all.

DR. MACMAHON: I think it would be appropriate for me on behalf of the participants to thank the Milbank Memorial Fund for the opportunity of participating in the meeting. It has been an extremely interesting one.

# THE INFLUENCE OF WAR AND POSTWAR CONDITIONS ON THE TEETH OF NORWEGIAN SCHOOL CHILDREN<sup>1</sup>

## IV. CARIES IN SPECIFIC SURFACES OF THE PERMANENT TEETH

GUTTORM TOVERUD,<sup>2</sup> LOUIS RUBAL<sup>3</sup> AND DOROTHY G. WIEHL<sup>2</sup>

**C**HANGES in the condition of the teeth of Norwegian school children during and after World War II have been described in earlier publications (Toverud, 1956, 1957, II and III) which reported the findings from nationwide examinations on 5,000 to 7,000 children each school year from 1940-1941 to 1948-1949 and in 1951-1952 and 1952-1953.

Main points discussed in these reports are as follows:

(1) Eruption time of the permanent teeth, particularly of teeth replacing deciduous molars, was "delayed" during the War and changed later toward prewar eruption patterns. This was ascribed chiefly to longer retention of deciduous teeth as a result of less carious destruction, and partly to a nutritional factor.

(2) Caries rates in deciduous and permanent teeth showed a sharp reduction during the War years and began to increase in 1946 or later. Rationing of foods, particularly the great reduction in sugar and products of sugar and white flour leading to infrequent eating of these substances, together with increased consumption of coarse bread, potatoes and vegetables were con-

<sup>1</sup> From the Pedodontic Department, Dental Faculty, University in Oslo, former Norwegian State Dental School, and the Milbank Memorial Fund.

<sup>2</sup> Dr. Philos., Oslo; F.D.S.R.C.S., England and Edinburgh; Professor of Pedodontia.

<sup>3</sup> Milbank Memorial Fund, research staff.

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sidered the main causes of the reduced caries rates. Changes in diet and eating habits reduced the factors conducive to tooth destruction and increased the possibilities for early post-eruptive maturation of enamel.

(3) It was shown (Toverud, 1957, II) that a definite reduction had taken place in total *surface* caries rates for permanent first molars in 12-13-year-old children during the War in spite of the fact that the DMF *tooth* rate did not show any reduction until 1948. This points to the value of studying changes in caries in the tooth surfaces in studies of the effect of caries preventive or caries promoting factors.

In the present report, trends of the specific surface caries rates during the War and postwar years will be examined for the different permanent teeth. First, changes in surface caries rates in children of a specific age examined in each of the nine school years from 1940-1941 to 1948-1949 will be described; and second, the accumulation of caries between age 8 and 12 years will be studied for quasi-cohorts of children reaching age 12 years at the end of the war period and in later years.

#### DATA AND METHOD

The data collected and methods of analysis for the Nor-

Table 1. Number of children<sup>1</sup> aged 7, 8, 12 and 13 years examined in villages in Norway 1940-1941 to 1948-1949, and in 1951-1952 and 1952-1953.

SCHOOL YEAR	AGE 7			AGE 8			AGE 12			AGE 13		
	Total	Boys	Girls	Total	Boys	Girls	Total	Boys	Girls	Total	Boys	Girls
1940-41	435	220	215	294	151	143	248	131	117	310	165	145
1941-42	388	184	204	368	180	188	224	121	103	282	145	137
1942-43	456	236	220	376	192	184	222	110	112	320	165	155
1943-44	333	182	151	400	212	188	262	140	122	371	193	178
1944-45	278	125	153	269	138	131	301	170	131	357	190	167
1945-46	354	187	167	410	201	209	363	172	191	416	228	188
1946-47	259	135	124	249	121	128	238	120	118	232	129	103
1947-48	339	162	177	300	158	142	283	146	137	293	150	143
1948-49	293	156	137	332	155	177	268	144	124	295	155	140
1951-52 <sup>2</sup>				174	92	82	167	85	81			
1952-53 <sup>2</sup>				238	120	118	176	82	94			

<sup>1</sup> Number with one or more permanent teeth erupted.

<sup>2</sup> Three villages only in 1951-1952 and 1952-1953. See footnote 4.



wegian dental study have been described in detail (Toverud, 1956). Only those aspects of the study which are of special relevance to this report are discussed here.

Examinations of the school children were made by school dentists, using a mirror and explorer. On a special form with a numbered square for each tooth, the dentist recorded whether the permanent tooth was erupted, present or extracted and indicated the surface location for any untreated caries and for any fillings. The examination was limited to the right side of the mouth.

Annual examinations in a randomized sample of Norwegian communities had been planned, but for a variety of reasons, particularly during the German occupation, not all communities in the sample were represented in every year. In this analysis of surface caries, combined data for ten villages with dental examinations in most years from 1941 to 1949 are used.<sup>4</sup> In most of the villages, only children in the first, second, sixth and seventh grades were examined with the result that only ages 7, 8, 12 and 13 years are well represented. The numbers of children at these ages examined each year are shown in Table 1.

The considerable annual variation in the numbers of examinations at each age is due not only to omission of data from one or more villages in some years but also to failure of some dentists to examine all the children in the specified grades every year. Consequently, at any specific age, examinations from the different villages were varying proportions of the total from year to year; and also in any year, examinations from a specific village were varying proportions of the total examinations at the different ages. The shifting composition of the

<sup>4</sup> Villages with data for 1941 to 1949 and for 1951-1952 and 1952-1953 are Baerum, Oppegård and Stord. Other villages included for the first period are: Aker, Eidsvoll, Hedrum, Odda, Ski, Strinda and Tune. The numbers of children of different ages examined in each village in the different years has been shown in Part I (Toverud, 1956). Villages with no examinations in certain years are: Stord, in 1945; Ski and Hedrum in 1947; Aker and Strinda, in 1947, 1948 and 1949. In addition, in Stord, examinations for 12 and 13 year-old children were not available for the years 1941 to 1945, inclusive.

population probably contributed to the irregular fluctuations in annual rates of caries prevalence at specific ages, but a careful check of the data from individual villages gave no indication that the overall pattern of changes in prevalence was significantly affected.

The longitudinal study of incidence of caries between two ages could not be limited to a definite cohort (specific population) because of the varying composition of the population by age and school year. However, a large majority of children examined at age 12 years in 1944-1945 and later were included in the population examined four years earlier at age 8. The increment in caries in specific surfaces of the first permanent molars and the incisors between these ages is studied for the War years and later.

For this report on trends in caries in specific surfaces and on increment in surface caries for cohorts of children, recorded annual rates for the population examined in the period 1941 to 1949 have been smoothed to eliminate irregular fluctuations. The procedure for obtaining smoothed rates was as follows:

1. A three-year moving average was used from the earliest year, 1941, to the year with the lowest recorded rate.
2. The rate for 1941 was obtained by extending the moving average trend (difference between 1942 and 1943) backward one year unless this estimate seemed to exaggerate the initial value. In these instances, either the observed rate was used for first year, or, if the 1942 rate was higher than the 1941 rate, the average for these two years was used for both years.
3. The year for the minimum rate in the smoothed values is that in which the recorded rate was a minimum. The "smoothed" minimum rate was obtained by extrapolating the moving average trend or using the observed minimum, whichever was *higher*.
4. Smoothed rates for years following the minimum rate are usually values obtained from a three-year moving average or estimated from a straight line based on the averages for two

successive two-year periods. When the minimum was in 1948, the recorded rate for 1949 was usually used.

The smoothed rates obtained by the above procedures give a conservative estimate of the decrease in caries between 1941 and the year with the lowest rate, and the latter is always the year in which the recorded rate was a minimum. Any increase in caries shown for years after the minimum rate also is a conservative estimate.

Rates used in this study are for boys and girls combined. The numbers of each sex at specific ages are shown in Table 1. The small difference in proportions by sex from year to year would have very little effect on the rates, and the effect is further reduced in the smoothed rates.

#### TRENDS IN SPECIFIC-SURFACE CARIES RATES 1940-1941 TO 1948-1949

Since, generally, caries involvement in the permanent dentition of younger school-age children is confined mainly to the first molars, patterns of change in the specific-surface caries rates for these teeth in the 7-year-olds will be examined first. Later, caries rates will be presented for the surfaces of the permanent incisors in 8-year old children and for surfaces of all the permanent teeth of the 13-year old children.

*First Molars of 7-Year Old Children.* The smoothed annual rates for the permanent first molars of the 7-year old children are shown in Table 2 and Fig. 1, the latter illustrating graphically the relative order of caries involvement in the different surfaces, as well as the trends.

As Fig. 1 clearly indicates, the surface with maximum caries involvement in both upper and lower first molars at this age is the occlusal. The rate for "any surface," i.e., the DMF tooth rate as conventionally used, is seldom more than one to three percentage points above the occlusal rate in the same year, so that the trend for first molar *tooth* DMF rates is essentially the same as the trend for the occlusal values. Minimum rates are shown for the distal surfaces of the upper molars

SCHOOL YEAR	UPPER TEETH						LOWER TEETH					
	Any Surface	Occlusal	Mesial	Distal	Buccal	Lingual	Any Surface	Occlusal	Mesial	Distal	Buccal	Lingual
PER CENT OF SURFACES CARIOUS IN FIRST MOLARS												
1940-41	68.2	67.5	3.7	1.3	8.2	15.9	78.4	76.2	9.1	3.9	29.7	4.0
1941-42	62.6	61.7	2.6	0.9	6.3	14.2	72.5	70.1	6.1	2.5	26.7	2.8
1942-43	57.0	55.9	1.5	0.5	4.4	12.5	66.6	64.0	3.1	1.1	21.2	1.6
1943-44	51.2	50.0	1.1	0.2	2.3	11.5	61.6	58.6	1.5	0.6	18.3	1.5
1944-45	44.6	43.4	1.1	0.2	1.5	8.8	54.1	51.0	1.2	0.5	15.4	1.4
1945-46	38.0	36.8	1.1	0.1	0.7	6.1	46.6	43.4	1.2	0.3	11.4	1.1
1946-47	40.0	38.0	0.8	0	0.4	9.0	50.2	47.0	0.8	0	13.7	0
1947-48	43.8	42.8	0.7	0.6	2.1	12.1	53.9	50.6	3.8	1.3	16.6	0.9
1948-49	47.6	47.6	0.7	1.1	3.3	13.9	57.6	54.2	3.8	1.4	20.2	0.9
MAXIMUM PERCENTAGE REDUCTION IN SURFACE RATES												
	44.3	45.4	81.1	100.0	95.1	61.6	40.6	43.0	91.2	100.0	61.6	100.0
MAXIMUM ABSOLUTE REDUCTION IN SURFACE RATES												
	30.2	30.7	3.0	1.3	7.8	9.8	31.8	32.8	8.3	3.9	18.3	4.0

Table 2. Per cent of specific surfaces with caries (DF) for permanent first molars of 7-year-old children. Smoothed annual rates, 1940-1941 to 1948-1949, for combined villages.

and for the distal or lingual surfaces of the lowers. Mesial values are consistently higher than the distal for both molars.

Rates for corresponding surfaces usually are higher in the lower first molars than in the upper, the notable exception being the lingual with much higher values in the upper molars. Buccal involvement in the lower first molars and lingual in the upper is greater than all other surfaces except the occlusal. The interchange in the order of buccal and lingual rates in the upper and lower first molars is a characteristic feature of the caries pattern for these teeth. The smallest differences in annual rates for corresponding surfaces in upper and lower first molars are for the distal and mesial surfaces and the largest for the buccal surface. The average annual difference between the buccal rates in upper and lower molars is about 16 per cent; for lingual rates, it is 10 per cent and for occlusal about 8 per cent.

Some decrease from the 1941 rates occurred for every type of

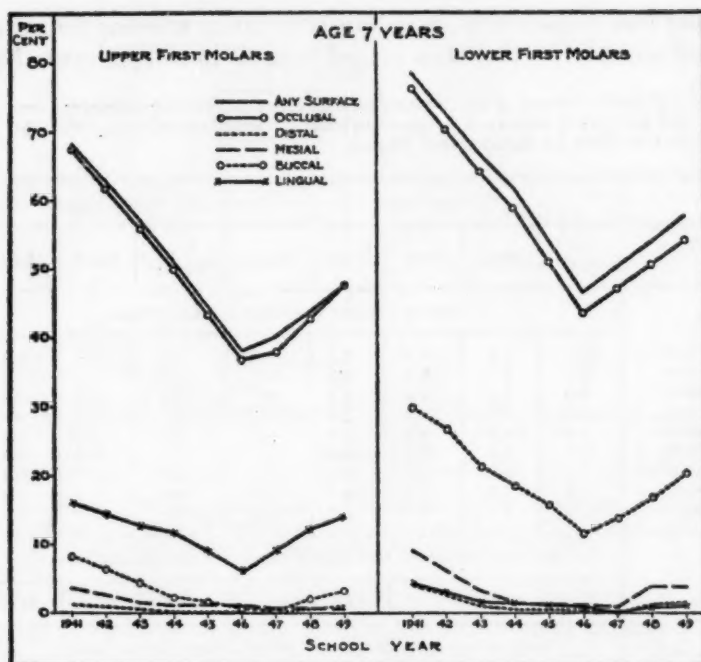


Fig. 1. For first permanent molars of 7-year-old children in villages, the percentage with any surface DMF and percentage of specific surfaces with caries (DF). Smoothed annual rates for school years 1940-1941 to 1948-1949.

surface of the first molars of the 7-year-old children. Also, in most instances an upturn in the curves is apparent in Fig. 1 after the low point was reached in 1945-1946 or 1946-1947, but the original levels were not attained by 1948-1949. The absolute reductions in rates for the different surfaces varied roughly according to the order of involvement within the given molar, the largest reduction being about 33 per cent for the lower occlusal and the smallest about 1 per cent for the upper distal surface. In terms of relative change, the maximum reductions were, by contrast, smaller for the occlusal rates than for the other surfaces. Thus, the minimum caries rates for the lower occlusal and for the upper occlusal surface were 43 and 45 per

cent less, respectively, than the 1941 rates; whereas for other surfaces relative decreases ranged from 62 to 100 per cent. For

Table 3. Per cent of specific surfaces with caries (DF) in the permanent central and lateral incisors of 8-year-old children. Smoothed annual rates, 1940-1941 to 1948-1949, for the combined villages.

SCHOOL YEAR	UPPER TEETH					LOWER TEETH*		
	Any Surface	Mesial	Distal	Buccal	Lingual	Any Surface	Mesial	Distal
PER CENT OF SURFACES CARIOUS IN CENTRAL INCISORS								
1940-41	10.3	3.8	4.6	2.5	1.5	5.8	4.1	2.1
1941-42	8.1	2.6	4.6	2.1	0.9	5.8	3.3	2.1
1942-43	6.9	2.2	4.4	1.2	0.5	3.5	2.3	1.4
1943-44	4.9	1.5	3.2	0.8	0.3	1.8	1.5	0.7
1944-45	2.9	0.8	2.0	0.4	0.3	1.2	0.8	0.5
1945-46	4.1	1.1	2.8	0.9	0.7	1.0	0.7	0.4
1946-47	5.3	1.3	3.7	1.1	0.7	0.9	0.5	0.4
1947-48	6.3	2.0	3.9	1.3	0.8	0.8	0.5	0.4
1948-49	6.5	2.7	3.9	1.1	0.6	0.8	0.3	0.5
MAXIMUM PERCENTAGE REDUCTION IN CENTRAL INCISOR SURFACE RATES								
	71.8	78.9	56.5	84.0	80.0	86.2	92.7	81.0
MAXIMUM ABSOLUTE REDUCTION IN CENTRAL INCISOR SURFACE RATES								
	7.4	3.0	2.6	2.1	1.2	5.0	3.8	1.7
PER CENT OF SURFACES CARIOUS IN LATERAL INCISORS								
1940-41	10.8	7.6	0.6	1.8	3.5	4.2	3.4	1.1
1941-42	10.8	7.6	0.6	1.1	2.7	3.2	2.2	0.7
1942-43	8.4	6.1	0.8	0.7	1.9	2.3	1.5	0.4
1943-44	5.7	4.1	0.4	0.3	1.5	1.2	0.7	0.1
1944-45	3.0	2.0	0	0	1.3	0	0	0
1945-46	5.6	4.4	0	0.4	1.5	0.5	0.5	0
1946-47	7.8	6.1	0.7	0.9	1.5	0.5	0.5	0
1947-48	8.3	6.7	0	0.9	1.4	0	0	0
1948-49	8.3	6.7	1.0	1.1	1.4	0	0	0
MAXIMUM PERCENTAGE REDUCTION IN LATERAL INCISOR SURFACE RATES								
	72.2	73.7	100.0	100.0	62.9	100.0	100.0	100.0
MAXIMUM ABSOLUTE REDUCTION IN LATERAL INCISOR SURFACE RATES								
	7.8	5.6	0.6	1.8	2.2	4.2	3.4	1.1

\* Buccal and lingual values not shown; no caries recorded in most school years.



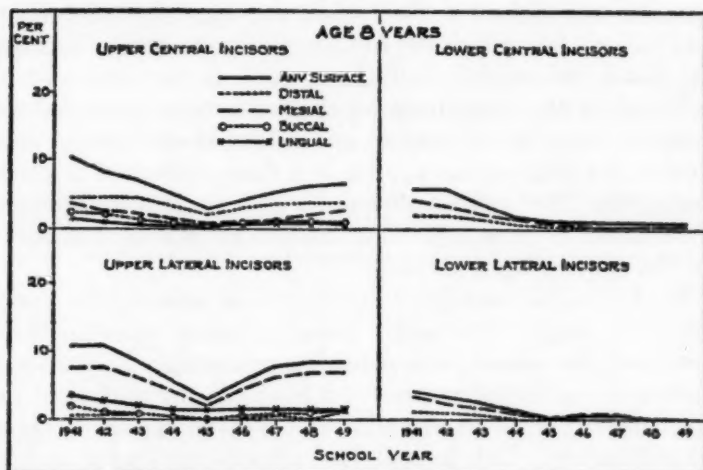


Fig. 2. For permanent incisors of 8-year-old children in villages, the percentage with any DF surface and percentage of specific surfaces with caries. Smoothed annual rates for school years 1940-1941 to 1948-1949.

the distal surface of upper molars and for both distal and lingual surfaces of lower molars, no caries were recorded for the children examined in 1946-1947, a reduction of 100 per cent. These zero rates should be viewed in the light of the very low rates of only 1 to 4 per cent in 1941. In general, the reductions percentagewise usually were greatest for the surfaces least involved, while the absolute reductions tend to be larger for the more susceptible surfaces.

*Permanent Incisors of 8-Year-Old Children.* The incisors are next to the first molars in order of emergence in the permanent dentition. The specific-surface caries rates in the incisors of the 8-year children given in Table 3 and Fig. 2 describe the caries pattern at an early stage of mouth exposure, and the changes in rates from 1940-1941 to 1948-1949. Caries rates for incisors generally are still rather low at this age but certain intradental patterns and trends appear to take fairly clear form.

It is evident from Fig. 2 that, intradentally, the mesial sur-

face had the highest caries rate in the upper lateral incisors and in both lower incisors; but, in the upper central incisors, the distal surface had the highest rate.

Trends in the caries rates for the contacting surfaces of the upper incisors (distal central and mesial lateral) were quite similar; for both surfaces, there is a sharp reduction in caries during the War years followed by a very definite increase. Caries rates in these contacting surfaces are discussed in detail for the children aged 13 years.

In the lingual surface of upper lateral incisors, the caries rates were higher than either distal or buccal rates for these teeth and also showed less reduction percentagewise than any surface of the lateral incisors, and less than any surface of the other incisors with the exception of the distal surface of upper central incisors. This pattern for caries in the lingual surface of upper lateral incisors is ascribed to the lingual pit.

With maximum rates for the incisors no more than 11 per cent at the beginning of the period, it is obvious that absolute reductions from the initial rates are small, but percentage reductions are large. The percentage reductions in any surface rates (tooth DF) varied from 100 per cent for the lower lateral incisors to 72 per cent for both upper incisors. Since the caries rates are so low for these teeth, significance of differences among rates for the specific surfaces and for the teeth derives largely from consistency in the patterns. In general, caries rates for the specific surfaces of upper incisors show a definite decrease to 1944-1945 and an increase thereafter; rates for the lower incisors were low initially and became negligible in later years.

**SPECIFIC-SURFACE CARIES RATES FOR THE DIFFERENT PERMANENT  
TEETH OF 13-YEAR-OLD CHILDREN, 1940-1941 TO 1948-1949**

The smoothed annual caries rates for specific surfaces of the different permanent teeth of 13-year-old children examined in the villages in 1940-1941 to 1948-1949 are presented in Table 4a, Table 4b and Fig. 3. For each type of tooth, the chart

shows the relative order of caries involvement for the different surfaces in any specified school year and also the changes over the nine-year period. The pattern of involvement and trends for each tooth may be compared with those of the adjacent or more remote members of the permanent dentition, but differ-

Table 4a. Per cent of specific surfaces with caries (DF) for the incisors and cuspids of 13-year-old children. Smoothed annual rates, 1940-1941 to 1948-1949, for the combined villages.

SCHOOL YEAR	UPPER TEETH					LOWER TEETH <sup>1</sup>			
	Any Surf.	Mes.	Dist.	Buc.	Ling.	Any Surf.	Mes.	Dist.	Buc.
CENTRAL INCISORS									
1940-41	65.0	42.8	51.0	5.0	6.5	21.5	16.9	16.0	0.9
1941-42	65.0	42.8	51.0	5.0	6.4	21.5	16.9	15.1	0.9
1942-43	62.4	40.0	48.3	4.7	6.0	21.6	17.3	14.2	0.6
1943-44	57.1	35.3	43.7	3.3	5.3	21.3	16.4	14.0	0.5
1944-45	51.0	29.7	39.6	3.0	4.4	19.3	14.9	12.7	0.5
1945-46	46.9	24.9	36.5	2.8	3.6	16.1	11.8	10.2	0.6
1946-47	44.7	21.6	34.7	2.6	3.5	11.3	7.8	6.7	0.7
1947-48	42.7	22.6	32.9	2.0	2.8	7.8	5.1	4.1	0.9
1948-49	44.4	23.2	35.9	1.5	2.7	5.6	3.7	2.9	0.9
LATERAL INCISORS									
1940-41	60.4	51.2	22.1	3.8	17.2	16.3	14.2	7.3	1.0
1941-42	60.2	50.3	20.3	3.8	17.2	16.3	14.2	7.2	1.0
1942-43	59.0	48.7	18.5	3.8	17.2	15.4	13.8	5.7	1.0
1943-44	54.9	44.5	15.3	3.3	16.3	14.1	12.9	4.2	0.6
1944-45	50.1	40.1	11.6	2.8	16.3	12.8	11.8	3.3	0.2
1945-46	46.5	36.4	8.6	2.3	16.1	9.4	8.5	2.7	0.4
1946-47	43.9	33.9	7.4	1.7	15.3	6.8	5.4	2.2	0.9
1947-48	41.3	31.4	6.6	1.1	12.1	4.7	3.5	1.8	1.1
1948-49	44.0	32.4	9.9	2.4	13.3	4.4	3.1	1.7	1.2
CUSPIDS									
1940-41	16.8	12.2	5.9	*	*	6.2	3.8	0.9	*
1941-42	16.8	12.2	4.1	*	*	6.2	3.8	0.9	*
1942-43	11.6	8.3	2.3	*	*	4.3	2.6	0.6	*
1943-44	7.2	5.0	1.8	*	*	2.0	1.3	0.3	*
1944-45	4.5	3.2	1.0	*	*	1.7	1.2	0.3	*
1945-46	2.5	1.6	0.3	*	*	1.3	0.8	0.3	*
1946-47	4.3	2.9	0.7	*	*	1.3	0.8	0.2	*
1947-48	6.5	4.8	1.6	*	*	1.0	0.6	0	*
1948-49	8.7	6.7	2.5	*	*	1.4	1.0	0	*

<sup>1</sup> Lingual rates omitted; less than 1.0 per cent and zero in many years.

\* Rates negligible.

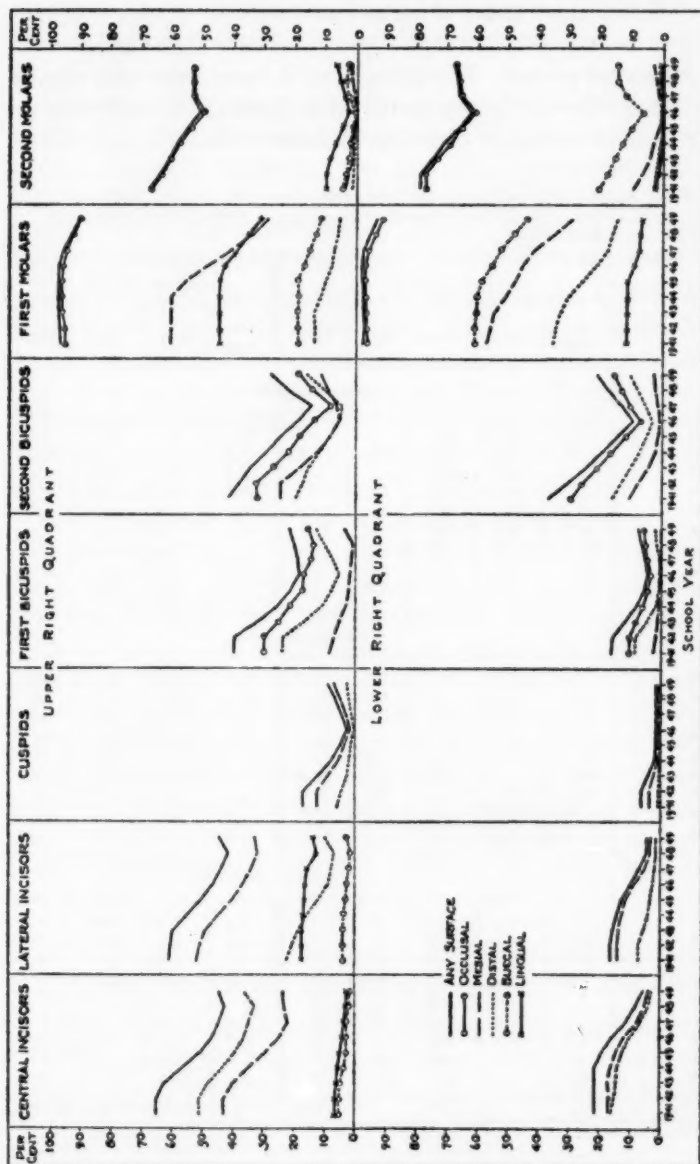


Fig. 3. For each permanent tooth of 13-year-old children in villages, the percentage with any DF surface and the percentage of specific surfaces with caries. Smoothed annual rates for school years 1940-1941 to 1948-1949.

Table 4b. Per cent of specific surfaces with caries (DF)<sup>1</sup> for the permanent bicuspid and molars of 13-year-old children. Smoothed annual rates, 1940-1941 to 1948-1949 for the combined villages.

SCHOOL YEAR	UPPER TEETH						LOWER TEETH					
	Any Surf.	Occl.	Mes.	Dist.	Buc. <sup>2</sup>	Ling. <sup>2</sup>	Any Surf.	Occl.	Mes.	Dist.	Buc. <sup>2</sup>	Ling. <sup>2</sup>
FIRST BICUSPIDS												
1940-41	39.7	29.6	8.4	23.8			15.9	10.4	2.7	8.4		
1941-42	39.7	29.6	6.8	23.8			15.9	10.4	1.9	8.4		
1942-43	31.8	25.1	5.2	16.3			11.9	8.6	1.1	4.9		
1943-44	25.4	21.3	3.3	10.4			8.0	6.0	0.9	2.2		
1944-45	21.1	17.4	2.5	7.7			5.6	4.4	0.8	0.9		
1945-46	18.2	16.7	1.8	5.3			4.3	3.3	0.9	0.3		
1946-47	18.9	14.8	1.4	7.6			5.5	4.4	0.5	0.9		
1947-48	20.3	13.8	1.0	10.1			6.6	5.2	0	1.7		
1948-49	21.7	15.5	3.5	12.6			7.6	5.8	0	2.5		
SECOND BICUSPIDS												
1940-41	41.8	32.4	24.9	19.7			37.1	29.9	10.6	16.3		
1941-42	38.2	32.4	24.9	17.6			31.6	26.4	7.7	13.9		
1942-43	34.5	27.1	17.9	15.6			25.1	21.4	4.9	10.5		
1943-44	28.7	21.9	11.8	11.5			19.8	17.0	2.5	7.7		
1944-45	24.5	18.6	8.6	8.8			14.7	12.1	1.7	5.0		
1945-46	18.8	13.7	5.4	5.7			9.6	7.2	0.9	3.2		
1946-47	14.7	8.8	6.5	4.9			13.5	10.3	1.5	5.2		
1947-48	22.1	14.1	9.1	10.9			17.6	13.1	2.2	7.7		
1948-49	27.7	18.8	11.7	16.9			21.3	15.4	2.9	10.2		
FIRST MOLARS												
1940-41	96.8	95.2	60.4	13.3	18.7	44.4	98.1	96.5	57.3	35.6	61.3	11.6
1941-42	96.8	95.2	60.4	13.3	18.7	44.4	97.8	96.6	56.1	34.6	61.3	11.6
1942-43	97.1	96.1	60.4	13.4	18.7	44.4	97.6	96.6	54.9	33.1	60.9	11.6
1943-44	97.2	96.3	60.4	11.4	18.7	44.4	97.8	96.9	50.7	30.6	60.4	11.6
1944-45	97.3	96.3	55.5	9.4	18.7	44.4	98.0	96.9	47.7	25.5	59.0	11.6
1945-46	96.9	95.9	47.8	7.7	17.0	43.1	98.0	96.9	45.6	21.8	55.7	9.5
1946-47	95.3	94.5	40.3	6.4	15.0	39.0	97.2	96.0	41.8	17.8	52.4	7.6
1947-48	92.9	92.0	33.2	5.4	13.2	34.8	95.2	93.6	35.5	16.3	48.2	7.4
1948-49	90.5	89.5	28.6	5.1	11.4	30.6	93.0	91.2	29.2	14.8	44.0	6.8
SECOND MOLARS												
1940-41	67.3	66.5	3.5	1.0	3.8	9.5	79.2	77.2	9.5	2.9	20.7	2.2
1941-42	64.0	63.3	2.3	0.8	3.0	9.5	79.2	77.2	7.0	2.2	18.0	1.5
1942-43	60.7	60.1	1.1	0.6	2.2	8.5	74.7	73.2	4.5	1.5	15.3	0.5
1943-44	58.6	57.9	1.3	0.5	1.8	6.6	71.1	69.8	3.0	0.7	13.3	0.7
1944-45	54.7	53.8	0.8	0.3	1.3	4.2	66.7	65.7	1.5	0.6	10.0	0.3
1945-46	50.8	49.7	0.3	0	0.8	2.1	62.3	61.6	0	0.5	6.7	0
1946-47	53.3	52.8	0.9	0	2.3	3.6	66.4	66.0	1.9	1.1	11.8	0
1947-48	53.4	52.9	1.3	1.3	3.1	5.0	67.0	66.5	2.8	1.4	14.7	0.2
1948-49	53.4	52.9	1.4	2.0	2.7	6.4	68.0	67.7	3.0	1.6	14.9	0.2

<sup>1</sup> Rates are percentages DF (decayed or filled) except for first molars. For the latter teeth the rates include extractions, see footnote 5.

<sup>2</sup> Buccal and lingual rates are omitted for bicuspid because they were negligible in all years.

ences in post-eruptive age, as well as in morphology, should be considered in such comparisons.

For the first molars, the percentages of carious surfaces are based on the total numbers of teeth including extractions and each specific-surface rate includes an estimated percentage of the extracted teeth.<sup>6</sup> For the other teeth, rates are percentages DF (decayed or filled); the numbers of extractions omitted were too small to affect the rates appreciably.

*Specific-Surface Caries Rates for First Molars.* The occlusal surfaces far outrank all others in the level of caries accumulation in both upper and lower first molars and, at age 13 years, the rates were near saturation for most of the period. However, for the other surfaces of each molar there is a wide variation in rates. Mesial rates tend to be relatively high, especially in the upper molar at the beginning of the study, when 60 per cent of the mesial surfaces were affected. In

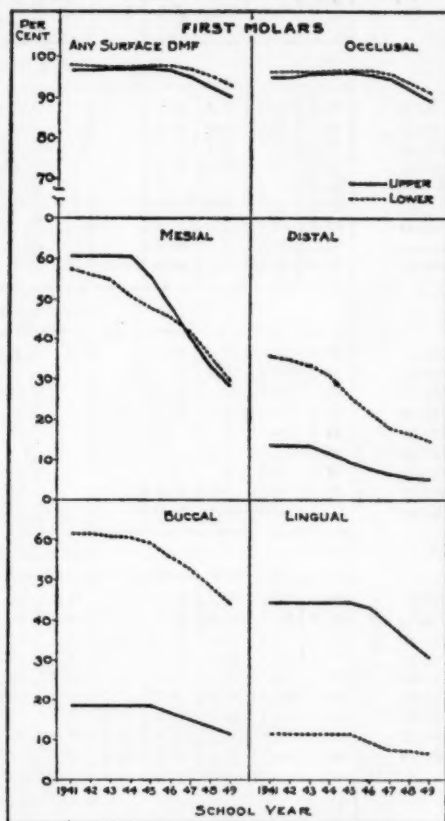


Fig. 4. Caries rates for specific surfaces of upper and lower first permanent molars of 13-year-old village children, 1941 to 1949.

<sup>6</sup> It was assumed that the occlusal surface was carious in all extracted first molars and that other surfaces were carious in the same proportions as in non-extracted teeth having both occlusal and other surface caries at the beginning of the study.



the lower tooth, buccal rates were even higher than mesial. The lowest values are provided by the distal surfaces of the upper teeth and by the lingual surfaces of the lower. The same interchange in order of lingual and buccal caries in the upper and lower first molars noted for the 7-year-old children is apparent also for children aged 13, with the lingual rate higher than the buccal in the upper molars and the buccal rate much higher than the lingual in the lower molars.

Caries rates for corresponding specific surfaces of the upper and lower first molars in 13-year-old children are compared in Fig. 4. It is apparent that the any surface rates as well as the occlusal rates were nearly the same in the upper and lower molars, rates for the lower being slightly higher. This is true during the total period 1941-1949. At age 7, the differences were substantially greater, indicating that, with increasing age and with values near 100 per cent, upper and lower occlusal rates tend to become indistinguishable. Mesial rates were higher in the upper molar from 1941 to 1946 and thereafter the rates for upper and lower molars were about the same. Distal rates were two and one-half times higher in the lower than in the upper molar in the early period but the differences decreased somewhat during the years after 1944. Rates for the buccal surface were nearly four times higher for the lower than for the upper molar during the whole period; and rates for lingual surfaces show differences of the same magnitude, but are higher for the upper tooth.

During this nine-year period, trends in the caries rates for different surfaces of the first molars obviously were quite dissimilar. Occlusal rates for both the upper and lower molars remained relatively unchanged at 95 per cent or higher until 1947 and the minimum rates in 1949 were only 5 or 6 per cent lower. These occlusal rates are closely equivalent to the any surface rates, that is, the tooth DMF rates which must be at least as high as the highest rate for any one of the specific surfaces. Thus, for these 13-year-old children, the DMF tooth rates represent occlusal values almost entirely and fail to reveal

large changes in rates for other surfaces. The changes in other specific-surface caries rates for the first molars fall roughly into the following pattern: Mesial and distal curves show the earliest and sharpest declines from the 1941 levels; buccal and lingual reductions are more delayed and less pronounced. The largest actual reductions are shown by the mesial rates which decreased by 32 per cent for the upper and 28 per cent for the lower molars. The maximum percentage reductions are found for the distal rates—62 per cent for the upper, 58 per cent for the lower. Reductions in the mesial rates were on the order of 50 per cent relatively; and those in the buccal and lingual rates were much smaller.

Although reductions in specific-surface rates for first molars relative to the 1940–1941 values occurred both at ages 7 and 13 years, the general pattern of change during the nine-year period is quite different. The curves at age 7 (Fig. 1) tend to be more or less saddle-shaped, pointing to an increase in caries near the end of the period. At age 13, no upturns are noted, and the reductions generally begin later.

*Specific-Surface Caries Rates for Second Molars.* The permanent second molars of 13-year-old children are generally at a relatively early mouth-exposure stage and one might expect a rather light caries accumulation. As can be seen readily in Fig. 3, the any surface rates for the second molars of the 13-year-old village children are usually as great as or greater than corresponding rates for all other permanent teeth except the first molars. However, this high level of DF rates for second molars is largely a reflection of the high occlusal involvement, other surfaces being much less affected.

The intradental pattern of surface caries in both the upper and lower second molars of the 13-year-olds may be compared with that shown earlier for the first molars of the 7-year-olds. Although the populations are different and the post-eruptive ages not quite the same, the strong morphological similarities in the two types of molars appear to be reflected in the relative order of surface caries within each tooth. Thus, in the upper

second molars the lingual surface rates are higher than all others except the occlusal; in the lower teeth the buccal values hold the same relative position. As in the case of the first molars of the 7-year-olds, in second molars of the 13-year-olds only the lingual rates are higher in the upper teeth than corresponding rates in the lower teeth.

The order of frequency of caries in specific surfaces of the second molars indicated for these Norwegian village children is the same as that found by Day and Sedwick (1935) for 13-year-old children (boys and girls combined). Also, for permanent first molars, the order is the same with the exception that, in the upper molars of the 13-year-old Norwegian children, caries were more frequent in the mesial than in the lingual surface during most of the years. Day and Sedwick reported caries in specific surfaces of first molars in the following order of frequency, lowest to highest: upper jaw—distal, buccal, mesial, lingual, occlusal; and lower jaw—lingual, distal, mesial, buccal, occlusal.

In general, the specific-surface caries rates for second molars decreased after the first year or two of the 1941–1949 period, reached a minimum in the school year 1945–1946 and then increased. This pattern corresponds exactly with that shown for first molars in 7-year-old children. However, the increase is not quite as sharply defined as for the first molars of 7-year-old children, especially for the occlusal surfaces. The absolute reductions were largest (about 16 per cent) for the occlusal surfaces, and only about one-half that for the corresponding surfaces of the first molars of the 7-year-olds (31–33 per cent), although initial rates were similar. The absolute reductions for the other surfaces of the second molars varied from 1 to 14 per cent, the smallest reductions occurring for the upper distal surfaces for which caries involvement was negligible in any year. Percentage-wise, however, the maximum reductions are smallest for the occlusal surfaces, being about 25 per cent for the upper and 20 per cent for the lower, whereas values up to 100 per cent are noted for other surfaces. Thus, it appears that

for both the first molars of the 7-year-olds and the second molars of the 13-year-olds the absolute decreases varied roughly according to the order of the specific-surface rates within the given tooth while maximum decreases percentagewise were smaller for the high-carries surfaces than for the low-carries surfaces.

*Specific-Surface Caries Rates for the Permanent Incisors.* There are large differences in the caries rates of upper and lower incisors, the latter pair having much less caries. However, while the tooth DF rates for adjacent incisors in the same jaw were at about the same levels and show similar trends over the period studied, the caries accumulation in the various surfaces appears to be different within each tooth, as may be seen in Fig. 3.

Of special interest are the patterns of caries in the different surfaces of the upper incisors. In the upper central incisor, distal surface rates are consistently higher than the mesial, the lower frequency in the latter surface being accounted for most probably by the diastema in the middle line of the upper jaw. The caries preventive effect of diastema on proximal surfaces has been clearly shown by Welander (1955) and Engh (1956). In the adjacent lateral incisors, mesial rates are far higher than distal, the low level of the latter being mainly attributable to the low caries rate of both the deciduous and the permanent cuspids. A comparison of the rates for the contacting surfaces of the upper incisors—distal central with mesial lateral—shows that they are almost identical in each year. The similarity of these rates at age 13 and also at age 8 years indicates that these surfaces acquire clinical caries at about the same time.

A decrease in caries rates for most surfaces of the upper incisors of the 13-year-old children began in the third year of the study, 1942–1943, and continued into 1947–1948, but in 1948–1949 the rates level off or rise slightly.<sup>6</sup> In general, surfaces

<sup>6</sup> For these smoothed annual rates, described in the section on method, the year in which the lowest rate was recorded has been accepted as the low point of the nine years. Considerable variation in rates can be attributed to population sampling and

(Continued on page 507)

with the highest caries rates show the smallest percentage reductions, as noted for other teeth. One notable exception to this relationship between caries level and percentage reduction, is the small decline in caries rates for the lingual surface of upper lateral incisors. For this surface, only a 30 per cent decrease is shown, compared with a 70 per cent decrease in the distal surface rates, although in 1941 the rates were 17 and 22, respectively, for the lingual and distal surfaces.

The intradental patterns of caries accumulation are somewhat different for the lower and upper incisors. In the lower central incisors, mesial and distal rates were almost equal, with the distal tending to be very slightly lower. (A diastema in the middle line in the lower jaw is very rare.) However, in the adjacent lower lateral incisors, caries rates for the mesial surface were highest and nearly the same as the distal rates in central incisors; and rates for the distal surface were much lower. A similar pattern was noted for the upper lateral incisors. The decrease in the mesial and distal rates for both lower incisors approximated 80 per cent and, at the end of the period, the rates were only 2 or 4 per cent. Buccal and lingual surfaces of the lower incisors had negligible rates, 1 per cent or less throughout the nine years.

The big differences between upper and lower incisors in the caries rates for corresponding surfaces are portrayed clearly in Fig. 5. For every surface, rates are much higher in the upper tooth than in the corresponding lower tooth surface during the entire period except rates for the buccal surfaces which at the end of the period had declined to the same very low level that was characteristic of buccal surfaces in the lower incisors throughout the period. In general, the high rates for the upper teeth began to decrease in 1942 or 1943 and a downward trend continued to 1947 or 1948, whereas for surfaces of the lower teeth, rates declined very little until after 1945 when there was

other sources of error variation, and consequently the minimum used may not have differed significantly from a previous or later year. Furthermore, when only one year, or even two years, followed the minimum, small increases in the rate do not firmly establish a reversal of trend.

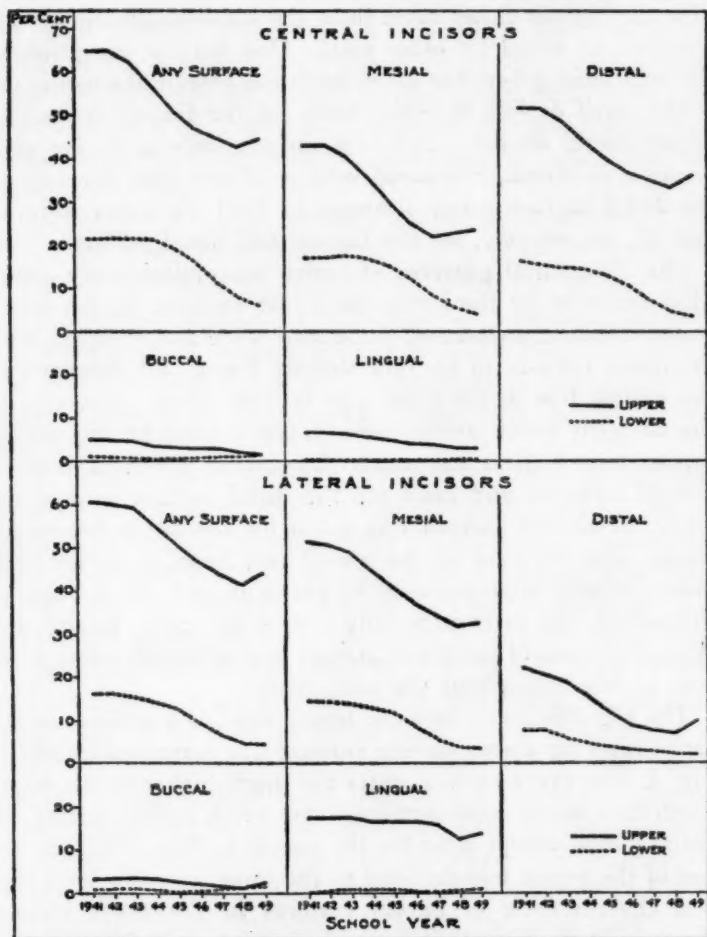


Fig. 5. Caries rates, per cent DF, for specific surfaces of upper and lower incisors of 13-year-old village children, 1941 to 1949.

a sharp reduction into 1949. Percentagewise, decreases in caries rates were greater for the lower than for the upper incisors. (See Table 5.) However, relative changes in rates for distal surfaces of the lateral incisors were fairly similar during the nine-year period, and the maximum decrease was 70 per cent



for the upper and 77 per cent for the lower lateral incisor.

*Specific-Surface Caries Rates for Cuspids.* The cuspids, among the least caries susceptible in the permanent dentition, have appreciable involvement only in the mesial and distal surfaces. Rates for mesial surfaces were consistently higher than for the distal, a difference corresponding to the higher distal surface caries rate of the adjacent lateral incisor than the mesial surface caries rate of the adjacent first bicuspid. Trends for the specific-surface caries rates were very similar during

Table 5. For 13-year-old children maximum reductions in caries rates for specific surfaces of the permanent teeth, 1940-1941 to 1948-1949. Estimated from smoothed annual rates; 1940-1941 rates the base year.

TOOTH	UPPER JAW						LOWER JAW					
	Any Surf.	Occl.	Mes.	Dist.	Buc.	Ling.	Any Surf.	Occl.	Mes.	Dist.	Buc.	Ling.
MAXIMUM PERCENTAGE REDUCTION IN CARIES RATE												
Central Incisor	34		50	35	70	59	74		78	82	•	•
Lateral Incisor	32		39	70	71	30	73		78	77	•	•
Cuspid	85		87	95	•	•	84		84	100	•	•
First Bicuspid	54	53	88	78	•	•	73	68	100	96	•	•
Second Bicuspid	65	73	78	75	•	•	74	76	92	80	•	•
First Molar	7	6	53	62	39	31	5	5	49	58	28	41
Second Molar	25	25	91	100	79	78	21	20	100	83	68	100
MAXIMUM ABSOLUTE REDUCTION FROM RATE IN 1940-1941												
Central Incisor	22.3		21.2	18.1	3.5	3.8	15.9		13.2	13.1	•	•
Lateral Incisor	19.1		19.8	15.5	2.7	5.1	11.9		11.1	5.6	•	•
Cuspid	14.3		10.6	5.6	•	•	5.2		3.2	0.9	•	•
First Bicuspid	21.5	15.8	7.4	18.5	•	•	11.6	7.1	2.7	8.1	•	•
Second Bicuspid	27.1	23.6	19.5	14.8	•	•	27.5	22.7	9.7	13.1	•	•
First Molar	6.3	5.7	31.8	8.2	7.3	13.8	5.1	5.3	28.1	20.8	17.3	4.8
Second Molar	16.5	16.8	3.2	1.0	3.0	7.4	16.9	15.6	9.5	2.4	14.0	2.2
CARIES RATE (PER CENT) IN 1940-1941												
Central Incisor	65.0		42.8	51.0	5.0	6.5	21.5		16.9	16.0	•	•
Lateral Incisor	60.4		51.2	22.1	3.8	17.2	16.3		14.2	7.3	•	•
Cuspid	16.8		12.2	5.9	•	•	6.2		3.8	0.9	•	•
First Bicuspid	39.7	29.6	8.4	23.8	•	•	15.9	10.4	2.7	8.4	•	•
Second Bicuspid	41.8	32.4	24.9	19.7	•	•	37.1	29.9	10.6	16.3	•	•
First Molar	96.8	95.2	60.4	13.3	18.7	44.4	98.1	96.5	57.3	35.6	61.3	11.6
Second Molar	67.3	66.5	3.5	1.0	3.8	9.5	79.2	77.2	9.5	2.9	20.7	2.2

\* Omitted; rates too low to evaluate changes.

the nine-year period; and the percentage reductions were 84 per cent or more. In the upper cuspids, there is a tendency for rates to increase after 1946; in the lower cuspids, values become negligible near the end of the period.

*Specific-Surface Caries Rates for Bicuspids.* For the different bicuspids it is evident in Fig. 3 and Table 4b that the trends for the tooth DF rates (any surface rates) roughly parallel the trends for caries rates for the principal surfaces affected—the occlusal, the mesial and the distal. Buccal and lingual caries rates were almost negligible and are not considered here. As in the case of the molars, the occlusal surface was the most highly affected in each type of bicuspids. However, for the bicuspids, differences between tooth DF rates and corresponding occlusal rates are considerable in contrast to very small differences for first and second molars, indicating that a considerable proportion of the carious bicuspids did not have occlusal involvement. Because of a difference in the fissure-system of bicuspids and molars, the occlusal surface of the bicuspids is less prone to caries.

Although the general level of any surface rates for the upper first and second bicuspids is about the same, the intradental mesial-distal patterns for the two types of teeth are different. For the upper first bicuspids, mesial rates are much lower than the distal, whereas for the upper second bicuspids, mesial and distal rates are fairly equal. The low caries rate in the mesial surface of first bicuspids corresponds with the low distal caries rate of adjacent cuspids already mentioned. In addition, the contact with its adjacent surface is different for the mesial and distal surfaces of first bicuspids and is less caries-disposing for the mesial surface. The contacting surfaces of the two upper bicuspids—distal surface of the first with mesial of the second bicuspid—have almost identical rates, indicating a probable similarity in individual rates of attack in the two surfaces as was noted for the corresponding situation in the upper incisors.

The well-known difference in susceptibility of the pair of lower bicuspids appears to carry over into the rates for the

specific surfaces. The order of involvement is, however, about the same in each tooth, the occlusal being higher than the distal and the latter higher than the mesial. The lower caries rate of the mesial surface than of the distal for first bicuspid and the similarity of rates for the contacting surfaces of the two bicuspid may be explained in the same way as for the maxillary bicuspid. The differences in occlusal rates are attributed to the differences in occlusal grooves in these two teeth.

For all surfaces of the bicuspid, reductions in caries rates had occurred by 1942-1943 or before, and a marked downward trend continued to 1946 or 1947. Thereafter, the rates for most surfaces increased. The largest absolute reductions in rates were 24 and 23 per cent for the occlusal surfaces of the upper and lower second bicuspid, respectively; and about 19 per cent for the adjacent distal and mesial surfaces of the two upper bicuspid. The smallest reductions were for the surfaces of the lower first bicuspid, being only about 3 per cent for mesial surfaces.

Percentage reductions tended to follow the usual pattern and were somewhat larger for surfaces with low caries involvement than for those with rates at higher levels for the same tooth. However, variations in percentage reduction were not very large for surfaces of the second bicuspid, 73 to 78 per cent among upper and 76 to 92 per cent among lower surfaces (see Table 5). Furthermore, the percentage reductions for occlusal surfaces of the four bicuspid did not vary in relation to initial caries rates. Thus, for upper first bicuspid, the maximum percentage reduction was 53 per cent compared with 73 and 76 per cent for occlusal surfaces of the second bicuspid, although all had approximately the same initial caries rate, and the maximum reduction was 68 per cent for the occlusal surface of lower first bicuspid which had much less caries involvement.

The differences in specific-surface caries rates between the upper and the lower bicuspid are demonstrated more clearly in Fig. 6. In the first bicuspid the rates for the upper tooth are more than twice as high as those for the lower one. Rates

for the mesial surface of the upper second bicuspid are also more than double those for the lower one, whereas the rates for the other surfaces of this tooth do not show the same big differences. The cause

of the bigger difference between caries rates for mesial surfaces of upper and lower second bicuspids may be found in the anatomy of the contacting distal surfaces of the first bicuspids. The distal surface of the lower first bicuspid has a typical convex form whereas the corresponding surface of the upper first bicuspid is flatter and thus gives a condition more conducive to plaque formation. Throughout the nine-year period, the trends for specific-surface caries rates for upper and lower first bicuspid were very similar. A corresponding parallelism is found for second

bicuspids except that rates for occlusal and distal surfaces of the lower teeth were a minimum in 1945-1946 and corresponding rates for the upper teeth continued to decrease until 1946-1947.

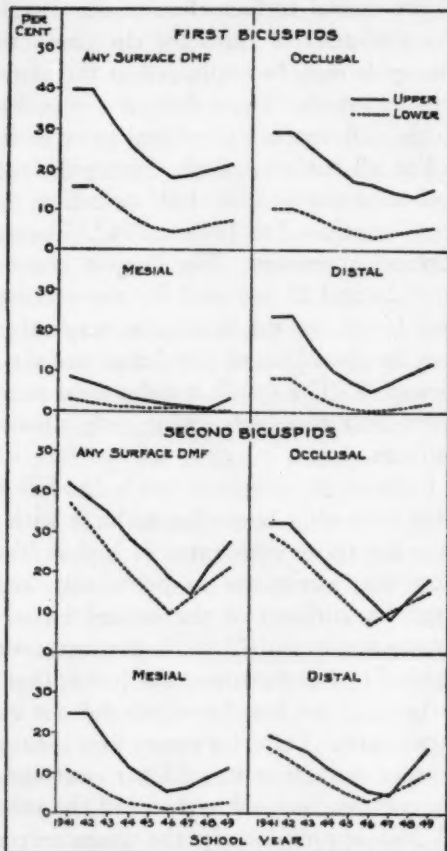


Fig. 6. Caries rates for specific surfaces of upper and lower first bicuspids and of second bicuspids of 13-year-old village children, 1941 to 1949.

SUMMARY DISCUSSION OF SPECIFIC-SURFACE CARIES RATES,  
1940-1941 TO 1948-1949

The foregoing analysis of caries rates for specific surfaces of the different permanent teeth of younger and older school children indicates several quite consistent patterns that support some general conclusions concerning intradental differences in surface involvement and concerning the effect of war conditions on the different permanent teeth.

First, it may be noted that the general differential involvement of specific surfaces of each tooth for children of a given age persisted with very few exceptions throughout the period. In other words, for most of the teeth there was no significant shift in order of rates, although the amount of difference among surfaces of a specific tooth did change and in some teeth, caries rates for some surfaces became zero or too low to indicate any significant surface difference. The outstanding shifts in the order of surface involvement occurred in the upper teeth of 13-year-old children as follows: (1) in the lateral incisors, caries rates for the lingual surface were lower initially than for distal surfaces but were higher after 1944 because there was almost no decrease in lingual caries; (2) in first molars, lingual rates were considerably lower than mesial rates until 1945, and thereafter were nearly equal; and (3) in the second bicuspid, distal rates were somewhat lower than mesial rates initially, but were equal in 1944 and later years. The shift in rank of the lingual surface of upper first molars and of the distal surface of second bicuspid is the result of relatively greater changes in caries in the mesial surface of these teeth. The pit in the lingual surface of upper lateral incisors already has been mentioned as a factor contributing to the small decrease in caries.

The comparative level of caries in the different surfaces of each tooth need not be described again. It is shown clearly in Figures 1, 2 and 3. For every tooth with an occlusal surface, this surface had the maximum caries rate. In the molars, rates for the occlusal surface were approximately equal to the tooth

DF rate (any surface DF) since it was rare for any other surface to be carious without occlusal involvement. In the case of the cuspids and both incisors, except upper central incisors, the mesial surface had the highest caries rate, and the distal rate ranked second (see previous paragraph for shift in order in upper lateral incisor). In upper central incisors, the distal surface rates were higher than mesial rates. Buccal and lingual surfaces had quite low caries rates in all the teeth with the exception of the molars and of the lingual surface in upper lateral incisors. In the upper molars, the caries rate in the lingual surface was second only to the occlusal rate for first molars at age 7 and second molars at age 13; and ranked third for first molars at age 13 except at the end of the period when it was second and slightly higher than the mesial surface. In the lower molars, caries occurred in the buccal surface more frequently than in any surface except the occlusal.

The amount of decrease in caries and the percentage reduction from the earliest available rate (1940-1941) varied widely for different surfaces in the same tooth and for the same surfaces of different teeth, although some decrease in caries was recorded within the nine-year period in every surface of each tooth. There were also differences in the year in which a downward trend became evident and the speed with which it progressed. These differences in trends are associated with two factors; first, the duration of mouth exposure of the tooth at time of examination, and second, the degree of susceptibility of the specific surface.

The general pattern of the trend was very similar for teeth which had had a fairly comparable period of mouth exposure. The teeth reported on here may be classified in two groups: *Group I*, teeth with short exposure at examination, including first molars at age 7, incisors at age 8, second molars, cuspids and bicuspids at age 13 years; and *Group II*, teeth with long exposure, including first molars and incisors at age 13 years.

For teeth in Group I, the annual caries rates usually show some decrease beginning in 1942 and reach a low level in 1945



(incisors) or 1946 (all others except upper second bicuspid which were at a minimum in 1947). In the following years, an increase occurred. The slope of the curves for specific surfaces usually is greatest for the surface having the highest rate; that is, for the surface most susceptible to caries. However, the larger absolute reduction in the caries rate for the most susceptible surface of a tooth represents, as a rule, a smaller proportional reduction than that occurring in the other surfaces. For surfaces with very low susceptibility, the caries rates became negligible and generally increased little or not at all at the end of the period.

The overall effect on the permanent teeth in Group I of changes in caries in the specific surfaces is described in Table 6 by the average number of carious surfaces per tooth with any caries.<sup>7</sup> An improvement in the carious condition of the dentition may be measured by a reduction in the frequency of multiple carious surfaces as well as by a reduction in the tooth DF rates. For the molars, the very susceptible occlusal surface largely determines the tooth DF rates, but less frequent involvement of the other surfaces of the molars is of great importance in preserving the tooth. For 7-year-old children, the average number of DF surfaces for lower first molars having any caries was 1.57 in 1941 and had decreased to 1.23 in 1946 as a result of a greater percentage decrease in all other surfaces (70 per cent) than in occlusal surfaces (43 per cent). A similar reduction in the number of carious surfaces per DF tooth is shown in Table 6 for the upper first molars, and also for second molars and bicuspid of 13-year-old children. For all these teeth, there is a striking similarity in the percentage reduction of caries rates for all surfaces exclusive of the occlusal; for the upper teeth, the reduction ranged from 73 to 82 per cent, and for the lower teeth from 70 to 87 per cent. The reduction was more variable for occlusal surfaces, being as low as 25 and 20 per cent in upper and lower second molars and as high as 73

<sup>7</sup> Since the cuspids had a low tooth DF rate and multiple carious surfaces were infrequent even in 1941, the number of carious surfaces per tooth are not shown in Table 6.

and 76 per cent in second bicuspid of 13-year-old children. The second bicuspid, which had the highest average number of DF surfaces per DF tooth, had the greatest decrease in the averages; for the upper tooth, the average decreased from 1.89

Table 6. Average number of carious surfaces per DF tooth; total carious surfaces for incisors, caries rates for occlusal surfaces and cumulated rates for other surfaces of permanent molars and bicuspid in 1940-1941, in year with lowest tooth DF rate, and in 1948-1949 and percentage change in rates from first year.

SPECIFIC TOOTH, AGE AND YEAR <sup>1</sup>	UPPER JAW					LOWER JAW				
	DF Surfaces per 100 Teeth		Per Cent Decrease from 1941		Av. No. of DF Surf. per DF Tooth	DF Surfaces per 100 Teeth		Per Cent Decrease from 1941		Av. No. of DF Surf. per DF Tooth
	Occl.	All Surf. Exc. Occl.	Occl.	All Surf. Exc. Occl.		Occl.	All Surf. Exc. Occl.	Occl.	All Surf. Exc. Occl.	
<b>Group I—Short Exposure</b>										
<i>Age 7: First Molars</i>										
1941	67.5	29.1			1.42	76.2	46.7			1.57
1946	36.8	8.0	45	73	1.18	43.4	14.0	43	70	1.23
1949	47.6	19.0	30	35	1.40	54.2	26.3	29	44	1.40
<i>Age 8: Central Incisors</i>										
1941		12.4			1.20		7.2			1.24
1945		3.5		72	1.21		1.5		79	1.25
1949		8.3		33	1.28		0.8		89	1.00
<i>Lateral Incisors</i>										
1941		13.5			1.25		5.1			1.21
1945		3.3		76	1.10		0		100	
1949		10.2		24	1.23		0		100	
<i>Age 13: Second Molars</i>										
1941	66.5	17.8			1.25	77.2	35.3			1.43
1946	49.7	3.2	25	82	1.04	61.6	7.2	20	80	1.30
1949	52.9	12.5	20	30	1.22	67.7	19.7	12	44	1.29
<i>Second Bicuspid</i>										
1941	32.4	46.5			1.89	29.9	29.8			1.64
1947, 1946	8.8	11.5	73	75	1.38	7.2	4.5	76	85	1.22
1949	18.8	28.8	42	38	1.72	15.4	13.4	48	55	1.35
<i>First Bicuspid</i>										
1941	29.6	33.5			1.39	10.4	13.0			1.47
1946	16.7	8.1	44	76	1.36	3.3	1.7	68	87	1.16
1949	15.5	16.9	48	50	1.49	5.8	3.0	44	77	1.16
<b>Group II—Long Exposure</b>										
<i>Age 13: First Molars</i>										
1941	95.2	136.8			2.40	96.5	165.8			2.67
1946	95.9	115.6		15	2.18	96.9	132.6		20	2.34
1949	89.5	75.7	6	45	1.83	91.2	94.8	5	43	2.00
<i>Central Incisors</i>										
1941		105.3			1.62		34.0			1.58
1946		67.8		36	1.45		22.6		34	1.40
1948, 1949		60.3		43	1.41		8.0		76	1.43
<i>Lateral Incisors</i>										
1941		94.3			1.56		22.8			1.40
1946		63.4		33	1.36		11.6		89	1.23
1948, 1949		51.2		46	1.24		6.7		71	1.52

<sup>1</sup> When two years are listed, the first is the year for the minimum rate in the upper jaw and the second year applies to the lower jaw.

<sup>2</sup> Rates for first molars at age 13 are DMF rates; adjustment for extractions is explained on p. 502.

<sup>3</sup> Year for lowest tooth DF rate (any surface carious), 1946 shown for comparison with teeth in Group I.

\* Change less than 1 per cent.

to 1.38 and for the lower, it decreased from 1.61 to 1.22. In the second molars, especially the upper teeth, multiple surfaces were carious less frequently than for any other tooth in Group I except the incisors, both at the beginning of the period and in the year with the minimum DF tooth rates. In the latter year, surfaces other than the occlusal in second molars were rarely affected with caries although the DF rates for occlusal surfaces had decreased relatively little. At age 13 years, the second molars are at an early stage of eruption and a large percentage of these teeth has only the occlusal surface exposed whereas the second bicuspid usually have the "normal" clinical crown exposed.

It is of interest that when caries rates increased after the War, the increase was as great or greater for other surfaces than for occlusal surfaces in the first and second molars and in second bicuspid, and the number of carious surfaces per DF tooth became nearly equal to the average number in 1941 for some of the teeth. Thus, the factors conducive to caries in the post-war years were causing a spread of caries to other surfaces of affected teeth as much or more than they were increasing the incidence of caries in the occlusal surfaces, or in any single susceptible surface.

The small increase at the end of the period in caries rates for occlusal surfaces of second molars of 13-year-old children has been noted previously. For these teeth, there was a much greater increase in other carious surfaces, and a considerable rise in the number of DF surfaces per carious tooth. Mouth exposure time for many of the second molars, especially the upper molars, has been only a few months at age 13 years (about 50 per cent of upper molars were erupted at age 12 and 75 to 80 per cent at age 13). The less favorable oral environment of the post-war years apparently had not speeded up the rate of developing caries in the susceptible occlusal surface but had affected the accumulation of caries in "other" surfaces, presumably in molars having relatively long mouth exposure.

In the incisors, there is a slow accumulation of caries, and

at age 8, caries rates for the specific surfaces are so low that small changes in the number of surfaces with caries are not significant. In the early years, the average number of DF surfaces per DF tooth varied from 1.20 to 1.25 for the four incisors. A sharp decrease in the tooth DF rates during the War and an increase in the post-war years for upper incisors was not accompanied by significant changes in these ratios of surfaces per tooth.

Teeth in Group II, first molars and incisors of children aged 13 years, differed from those in Group I in the trends for caries rates during the nine-year period. A real decrease in rates began only in the third year or later, as late as 1946-1947 in the case of first molars. For most surfaces of these teeth, the downward trend continued into 1948-1949; for a few, there is a tendency for the rate to increase slightly in the last year.

What is the explanation for this principal difference in the two groups? One factor of importance is the oral environment at the time the tooth erupted. A second factor is the level of caries already present at the beginning of the period.

In an earlier report on this study (Toverud, 1957 III) it was pointed out that the oral environment during the first post-eruptive period is a decisive factor affecting the susceptibility of the tooth to caries.

The teeth in Group I are young teeth having been in contact with the oral environment for one to three years, whereas those in Group II have had a mouth exposure time of five to seven years. Most of the teeth in Group I examined in 1942 erupted after the rationing of food—particularly of the refined carbohydrates—had started, and thus erupted into an environment less harmful to the teeth than did the teeth examined in 1941. The environment continued to improve as a result of the restricted diet as long as the war lasted. The environment of the teeth not only comprises the destructive factors, carbohydrates and bacteria, but also protective forces through saliva. The latter had better opportunity for action during the period of restricted intake of refined carbohydrates. The condition in

the mouth was more favorable for the very important maturation of the enamel soon after eruption. This factor is no doubt the cause of the lapse of one to three years from the war's end to the rise in the caries rates. But the teeth examined in 1947-1949 which had erupted after the war, did not have the same post-eruptive maturation condition. These young teeth, therefore, responded quickly to an increasingly unfavorable environment.

Teeth of the second group with a mouth exposure time of five to seven years, of course, had accumulated different amounts of caries at the time of examination. At nine years of age children examined in 1940-1941 had caries in more than 90 per cent of occlusal surfaces of first molars. Consequently, nearly all first molars in 13-year-old children examined from 1941 to 1945 or 1946 had occlusal surface caries when the rationing started, and no decline in occlusal rates could be expected during these years. Also, since occlusal surface rates are nearly equal to the any surface rates, i.e. DMF tooth rates, the latter could not decline. However, these rates show a downward trend for 1947 and the two following years. In lingual and buccal surfaces, caries rates continued at a constant level until 1945 and then decreased rather sharply, as shown in Fig. 3. An earlier reduction in caries is shown for mesial and distal surfaces.

As shown in Table 6, the accumulation of caries in surfaces other than occlusal had decreased by 1946 and a reduction of 15 per cent is shown for upper first molars and 20 per cent for lower first molars, although no change in occlusal caries had occurred. With a reduction in the number of carious surfaces, there was less destruction of first molars at the end of the war period, and this type of improvement increased up to 1949.

For incisors of 13-year-old children, the sharp downward trend in caries rates for the different surfaces began in 1943 or 1944, except for lingual surfaces of upper lateral incisors. Prevalence of caries in these teeth increases gradually with increas-

ing post-eruptive age and the beneficial oral environment during the war years slowed the rate of accumulation of caries in teeth that had erupted before rationing. However, from 1941 to 1946 the reduction in carious surfaces was 36 and 33 per cent for upper central and upper lateral incisors, respectively, and was much less than for the younger teeth in Group I, the reduction for the same teeth being 72 and 76 per cent at age 8 years.

The continuous post-war decrease or leveling off of DF rates for first molars and incisors of 13-year-old children does not indicate that an increase in the rate of acquiring caries had not occurred in that part of the post-eruptive period that followed the war years. The teeth examined in 1948 and 1949 had erupted during the war years when, as has been pointed out, very little caries was accumulated in the first two or three post-eruptive years. Even a fairly large increment in later years could occur without increasing the caries prevalence at age 13 years above that for the preceding school year. Changes in the increments in caries in specific surfaces between two ages within different time periods are described in the next section.

#### TRENDS IN INCREMENTS OF CARIES IN SPECIFIC SURFACES OF PERMANENT FIRST MOLARS AND INCISORS

The increment of caries within a specific age interval affords a measure of caries activity which relates to a fairly constant post-eruptive age of a specific tooth and to a definite time period, that is, to the specific years in which the age interval occurred. Prevalence rates, on the other hand, are a measure of the accumulated incidence of caries during the total post-eruptive exposure of a tooth, and, if this exposure has been a number of years, the rates may not reveal changes in the age pattern of incidence of caries during the post-eruption period. Ideally, the increment should be obtained from repeated examinations of the same children (cohort) at successive ages, and a series of cohorts should be followed through different time periods in order to study time changes in increments of caries. Observed variations in increments among different cohorts then



can be identified with definite time periods and be related to any changes in conditions that might have affected caries activity.

For the 12-year-old children in the combined village group, a study was made of the time trends in increments of caries during the age interval 8 to 12 years for specific surfaces of permanent first molars and incisors. These four-year increments are not obtained from true cohorts of children, since, as previously explained, not all the 8-year-old children were examined in later years and not all the 12-year-old children examined in 1944-1945 or later had been examined earlier at age 8 years. However, the caries rates for the two ages are believed to be representative of the village group in the year of examination. Increments for a shorter interval than four years would be desirable in order to associate changes with specific years during the war and early post-war years when conditions affecting oral environment underwent rapid change; but data were available for few children aged 9 to 11 years. As a result of the changing trends in caries rates at specific ages, the age patterns of accumulating caries from 8 to 12 years no doubt differed markedly for the various cohorts, and within a four-year interval both low and high age specific incidence rates may have occurred.

In order to present some data on increments of caries in teeth which erupted in the post-war period, examinations made in three of the villages in 1951-1952 and 1952-1953 have been utilized for this study,<sup>8</sup> in addition to the examinations from the larger village group for the earlier period from 1941 to 1949. Data for the three villages, though not strictly comparable with that for earlier years, probably are fairly representative of the larger group. However, the caries rates for this smaller population are subject to much random variation.

*Increments of Caries for First Molars.* The annual rates for

<sup>8</sup> The three villages with examinations in 1951-1952 and 1952-1953 contributed one-half or more to the population of the total group in the earlier period 1941 to 1949. The actual computed rates are used for 1952 and 1953, but smoothed annual rates are used for the earlier period.

Table 7. Per cent of specific surfaces with caries for the permanent first molars of 8 and 12-year-old children examined in the villages in 1940-1941 to 1948-1949 and 1951-1952 and 1952-1953, and increments in per cents of surfaces carious between ages 8 and 12. Smoothed annual rates 1941 to 1949.

TOOTH SURFACE AND AGE	SCHOOL YEAR										
	1941	1942	1943	1944	1945	1946	1947	1948	1949	1952*	1953*
UPPER FIRST PERMANENT MOLARS											
<i>Any Surf. (DMF Teeth)</i>											
Age 8	79.9	79.9	71.0	64.3	56.6	50.0	52.9	59.7	66.5	66.3	68.9
Age 12	96.1	96.1	95.7	95.9	95.2	94.7	91.9	88.5	85.1	90.4	93.2
Incr. 8 to 12	(16.2)				15.3	14.8	20.9	24.2	28.5	30.7	26.7
<i>Occlusal Surf.</i>											
Age 8	78.2	78.2	70.3	63.4	55.4	47.8	51.0	58.1	65.0	65.7	68.9
Age 12	92.8	92.8	93.6	94.2	94.4	93.8	91.0	87.3	83.6	88.0	90.3
Incr. 8 to 12	(14.6)				16.2	15.6	20.7	23.9	28.2	29.9	25.3
<i>Mesial</i>											
Age 8	9.7	6.7	4.9	3.1	1.3	1.7	2.5	3.5	4.5	4.1	5.1
Age 12	55.3	54.7	53.8	47.6	41.2	34.7	27.4	20.1	21.3	32.5	43.1
Incr. 8 to 12	(45.6)				31.5	28.0	22.5	17.0	20.0	29.0	38.6
<i>Distal</i>											
Age 8	3.1	2.5	1.9	1.3	0.6	0	0	0	1.2	1.7	2.1
Age 12	10.7	10.5	8.9	8.0	6.3	5.2	4.5	3.7	2.9	5.9	9.0
Incr. 8 to 12	(7.6)				3.2	2.7	2.6	2.4	2.3	5.9	7.8
<i>Buccal</i>											
Age 8	13.4	11.8	8.7	6.6	3.9	2.5	2.0	4.6	7.1	7.0	11.1
Age 12	17.8	17.8	18.1	17.7	16.7	14.3	12.2	10.9	9.6	12.6	19.2
Incr. 8 to 12	(4.4)				3.3	2.5	3.5	4.3	5.7	8.0	12.1
<i>Lingual</i>											
Age 8	20.9	18.9	16.9	14.5	12.3	10.5	13.1	15.9	18.8	17.4	20.9
Age 12	41.1	41.1	42.9	41.8	37.3	35.3	30.9	26.5	27.0	26.7	33.8
Incr. 8 to 12	(20.2)				16.4	16.4	14.0	12.0	14.7	10.8	15.0
LOWER FIRST PERMANENT MOLARS											
<i>Any Surf. (DMF Teeth)</i>											
Age 8	89.1	89.1	81.7	75.1	68.5	64.3	61.1	69.0	74.5	79.8	83.5
Age 12	97.3	97.3	97.7	97.9	97.8	97.4	95.4	92.2	87.3	89.2	94.9
Incr. 8 to 12	(8.2)				8.7	8.3	13.7	17.1	18.8	20.2	20.4
<i>Occlusal Surf.</i>											
Age 8	86.5	86.5	79.3	71.9	65.5	59.5	59.5	65.7	71.9	76.3	81.8
Age 12	95.8	95.8	96.2	96.7	96.3	95.9	93.5	89.5	85.5	83.2	93.2
Incr. 8 to 12	(9.3)				9.8	9.4	14.2	17.6	20.0	17.5	21.3
<i>Mesial</i>											
Age 8	16.9	16.9	9.4	4.2	1.9	3.2	4.5	6.0	7.5	10.5	12.1
Age 12	52.7	52.7	47.7	42.6	39.0	34.8	28.8	22.8	24.8	43.1	56.3
Incr. 8 to 12	(35.8)				22.1	17.9	19.4	18.6	22.9	37.1	48.8
<i>Distal</i>											
Age 8	7.2	7.2	4.1	1.5	1.2	0.8	1.0	1.3	2.2	2.3	4.3
Age 12	29.1	29.1	26.5	20.3	17.4	13.9	11.7	9.5	11.5	19.4	17.1
Incr. 8 to 12	(21.9)				10.2	6.7	7.6	8.0	10.3	18.1	14.9
<i>Buccal</i>											
Age 8	37.7	32.9	28.1	25.1	21.1	17.1	19.9	23.6	25.4	22.1	24.6
Age 12	57.2	57.2	58.4	57.4	55.4	51.7	46.1	40.5	44.5	34.4	50.5
Incr. 8 to 12	(19.5)				17.7	18.8	18.0	15.4	23.4	10.8	25.1
<i>Lingual</i>											
Age 8	6.1	6.1	4.4	2.8	2.4	1.9	1.1	0.3	1.5	1.7	3.0
Age 12	9.2	9.2	9.2	9.2	7.2	6.6	6.7	5.5	4.3	4.3	4.5
Incr. 8 to 12	(3.1)				1.1	0.5	2.3	2.7	1.9	4.0	3.0

( ) Expected increment, assuming no significant trend in age-specific rates from 1937 to 1941.  
\* Reduced population, only 3 villages.

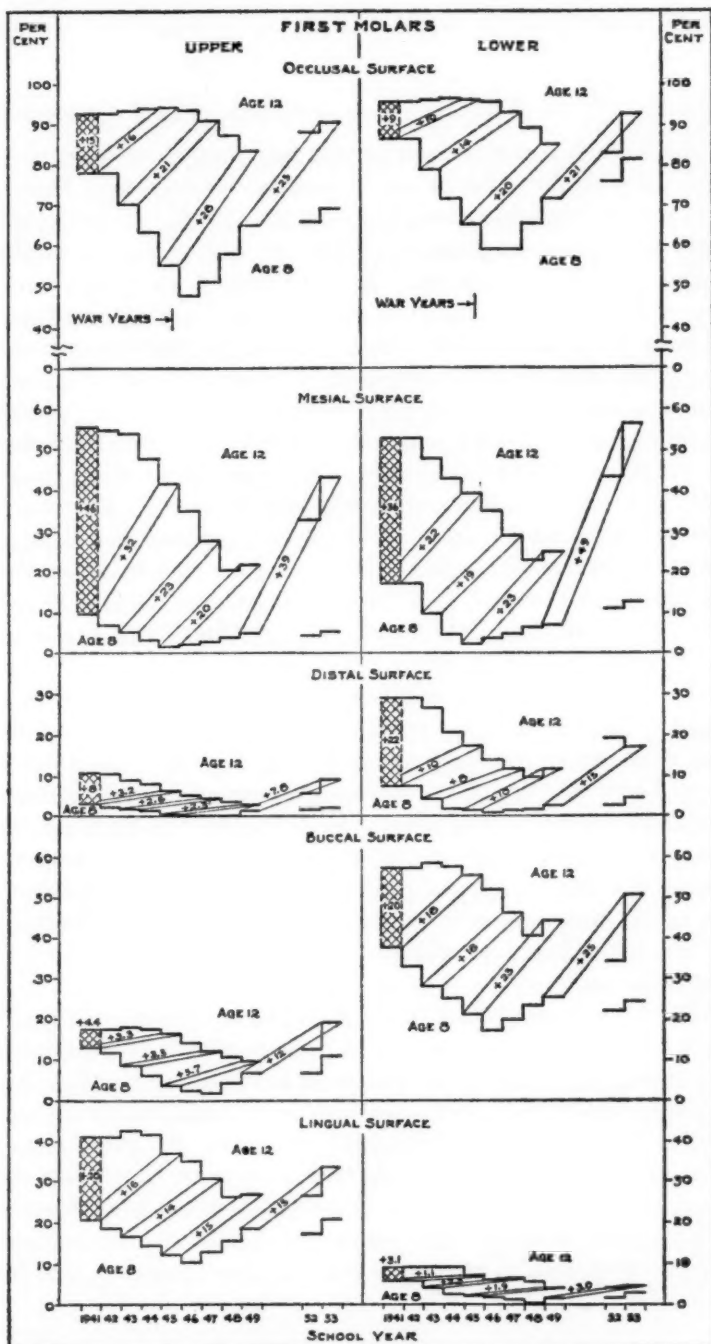


Fig. 7. Increments of caries in specific surfaces of permanent first molars during selected four-year periods from age 8 to 12 years, and for 8- and 12-year-old children, smoothed annual DMF rates for school years 1941 to 1949, and actual rates in 1951-1952 and 1952-1953 for a reduced population.

caries in each surface of permanent first molars for 8-year-old and 12-year-old children are shown in Table 7 and Fig. 7. For each 8-year-old cohort, the increment of caries during the next four years, as estimated from the rate at age 12 in the school year four years later, is also given in Table 7. Increments for selected cohorts are indicated in Fig. 7 by lines connecting the rates for corresponding 8- and 12-year-old children. For comparison with the longitudinal increments, the difference between rates in 1941 for 8-year-old and 12-year-old children is shown for each tooth surface. This difference may be taken as the usual or "expected" increment assuming no trend in age specific rates in the four years preceding 1940-1941. Data from school dental clinics do not indicate any appreciable changes in caries prevalence before the War. As noted in an earlier publication (Toverud, 1957, II), the number of DMF surfaces per 7-year-old child in Oslo was 6.7, 6.6, and 6.7 for the years 1938, 1939, and 1940.

Trends in the annual specific surface caries rates for children aged 8 and 12 years during the nine years from 1941 to 1949 are very nearly the same as the trends described for children aged 7 and 13. For the 8-year-old children, there was a sharp reduction in caries until 1945 or 1946, after which there was an increase. For 12-year-old children, the decrease in caries rates began later and continued until 1948 or 1949. The caries level at age 12 in any year is clearly the result of rapidly changing rates at age 8 and of changing increments between 8 and 12 years of age.

In the occlusal surfaces of both upper and lower molars, the increment for the 1941 and 1942 8-year-old cohorts was equal to or slightly greater than the expected increment and consequently the caries rate at age 12 showed no decrease in 1945 and 1946. For later cohorts, the increment is increasing but the increase is less than the decrease at 8 years of age and the occlusal caries rate at age 12 decreased in the post-war years. By 1953, the occlusal caries rate at age 12 had increased almost to the 1941 rate in part as a result of the increased rate for

8-year-old children examined in 1949 and in part as a result of a large four-year increment.

For mesial and distal surfaces of the first molars, the four-year increment of caries for the 1941 cohort was very much less than the expected increment and consequently the surface caries rates at age 12 were much lower in 1945 than in 1941. For successive cohorts of 8-year-old children including those examined in the school year 1944-1945 the increment to age 12 either continued to decrease or changed only slightly, and these increments combined with the low caries rates at age 8 produced a continued decrease in the rates for 12-year-old children up to 1948 or 1949. For the 1949 cohort, a sharp increase is shown for the increments of caries in these surfaces and a very high caries rate at age 12 in 1953 is indicated, as was noted for occlusal surfaces.

For buccal and lingual surfaces of upper and lower first molars, the increment of caries for the 1941 cohort was slightly less than the expected increment from age 8 to 12 years. Changes in increments of caries between age 8 and 12 years were generally small and the continued decrease in caries rates for these surfaces in first molars of 12-year-old children during the years from 1945 to 1949 was largely the result of the greatly reduced rates at age 8 during the war years. In 1949, caries rates in the buccal surfaces had increased considerably for 8-year-old children, and the increments in the next four years also increased, so that for 12-year-old children in 1953, the rate for the buccal surface was as high in upper molars and nearly as high in lower molars as in 1941. In the lingual surface of upper molars, caries rates increased for 8-year-old children in the post-war years, but the four-year increment remained constant. In the lower molars, lingual surface rates, which are very low even at age 12, became negligible for the 8-year-old children and remained so until 1949. The four-year increment of caries was 3 per cent for the 1949 cohort, the same as the increase from 8 to 12 years of age indicated by the 1941 examinations.

Changes in the increments of caries in the different surfaces

during the war and post-war years followed several patterns which are clearly associated with levels of susceptibility of the surface as indicated by the caries rates at age 8 years and the accumulation of caries by age 12 in 1940-1941. For occlusal surfaces, rates were already about 80 per cent or higher for 8-year-old children in 1940-1941 and the few surfaces not affected became carious in the next four years at the prewar or expected rate. A rapid decrease in occlusal caries rates during the war years for 8-year-old children was not accompanied by any decrease in the increments; in fact there was some increase beginning with the cohort that was 12 years old in 1946-1947. A similar pattern is found for buccal surfaces in lower molars for which the caries rate at age 8 also was relatively high initially and decreased sharply during the war years. The continuous large decrease in caries rates among 8-year-old children for these highly susceptible surfaces left increasing percentages of caries-free surfaces to be exposed to the post-war environment during the age interval 8 to 12 years and consequently the caries incidence increased in this age period. A different trend is found for surfaces with a low caries rate at age 8 and a large expected increment in the four years to age 12, notably the mesial surfaces and lower distal surface. For these surfaces, a sharp reduction from the expected increment began during the war years and persisted for the immediate post-war period, 1945 to 1949. Only in the later period after 1949 was there an increased increment for the ages 8 to 12 years. Although caries rates in the distal surface of upper molars were much lower at age 8 and 12 years the pattern for increment changes is similar to that noted for the lower distal surface. Changes in increments were small for upper and lower lingual surfaces and for the upper buccal surface except that in the latter surface caries increased sharply in the 1949 to 1953 period.

In an earlier report (Toverud, 1957 III) it was noted that the increments of caries for the total surfaces of permanent first molars between ages 8 and 12 years, and also the incidence



among caries-free surfaces, had decreased very little during the period 1941 to 1949. When specific surfaces are considered, it is apparent that for some surfaces there was a significant decrease in the increments.

If the increments of caries are expressed as incidence of new caries in surfaces free of caries at age 8, some of the surfaces have a different pattern of change during the period. These incidence rates are given in Table 8 and the trends are clearly shown in Fig. 8. Occlusal surfaces are of special interest. For the successive cohorts reaching age 12 in 1945 to 1947, there is

Table 8. Incidence of new caries in specific surfaces of permanent first molars during four years from age 8 to 12 years. Caries rate per 100 caries-free surfaces at age 8 years.

SCHOOL YEAR OF EXAMINATION		CARIES INCIDENCE (4 YEARS) PER 100 CARIES-FREE SURFACES				
At age 8	At age 12	Occlusal	Mesial	Distal	Buccal	Lingual
UPPER MOLARS						
1940-1941	1940-1941 <sup>1</sup>	67	51	7.8	5.1	26
1940-1941	1944-1945	74	35	3.3	3.8	21
1942	1946	72	30	2.8	2.8	20
1943	1947	70	24	2.7	3.8	17
1944	1948	65	18	2.4	4.6	14
1945	1949	63	20	2.3	5.9	17
1947-1948	1951-1952	71	30	5.9	8.4	13
1948-1949	1952-1953	72	40	7.9	13.0	18
LOWER MOLARS						
1940-1941	1940-1941 <sup>1</sup>	69	43	24	31	3.3
1940-1941	1944-1945	73	27	11	28	1.2
1942	1946	70	22	7	28	0.5
1943	1947	69	21	8	25	2.4
1944	1948	63	19	8	21	2.8
1945	1949	58	23	10	30	1.9
1947-1948	1951-1952	51	40	18	14	4.0
1948-1949	1952-1953	76	53	15	34	3.0

<sup>1</sup> Incidence based on difference between rates for 8-year-old and 12-year-old children examined in same school year.

a slight decrease in incidence of caries for both upper and lower molars, and for those becoming age 12 in 1948 or 1949 the decrease is accelerated, especially for lower molars. Although the incidence in these latter years occurred during the post-war period, the rate remained relatively low for these teeth which erupted during the last years of the war. For children 8 years old in 1949, incidence of caries in occlusal surfaces increased to the prewar level or higher during the four years ending in 1953.

For surfaces other than occlusal, the four-year incidence of caries in caries-free surfaces does not differ very greatly from the absolute increment, since the accumulated prevalence at age 8 is not large. Incidence rates for the mesial and lingual surfaces of upper molars and for the mesial and buccal surfaces of lower molars were at the lowest level for the four-year period 1944 to 1948; and in the next four-year period, these rates show an increase.

*Increments of Caries for Incisors.* Annual caries rates for

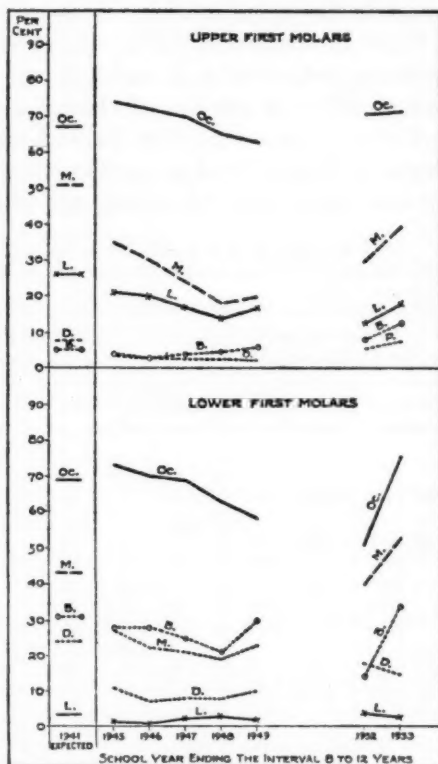


Fig. 8. For caries-free surfaces in permanent first molars of 8-year-old children, the incidence of caries in each surface in the four-year period to age 12 years. Expected incidence is estimated from rates in 1940-1941 for 8- and 12-year-old children.

Table 9. Per cent of specific surfaces with caries for the permanent central incisors of 8- and 12-year-old children examined in the villages in 1940-1941 to 1948-1949 and in 1951-1952 and 1952-1953, and the increments in per cents of surfaces carious between ages 8 and 12. Smoothed annual rates, 1941 to 1949.

TOOTH SURFACE AND AGE	SCHOOL YEAR											
	1941	1942	1943	1944	1945	1946	1947	1948	1949	1952*	1953*	
UPPER CENTRAL INCISORS												
<i>Any Surf. (DF)</i>												
Age 8	10.3	8.1	6.9	4.9	2.9	4.1	5.3	6.3	6.5	6.3	9.2	
Age 12	57.4	55.6	50.5	46.2	40.8	37.7	34.6	38.3	42.0	35.9	36.9	
Incr. 8 to 12	(47.1)				30.5	29.6	27.7	33.4	39.1	29.6	30.4	
<i>Mesial</i>												
Age 8	3.8	2.6	2.2	1.5	0.8	1.1	1.3	2.0	2.7	2.5	2.6	
Age 12	34.4	33.4	29.1	26.0	20.7	16.5	16.9	18.1	19.3	20.4	15.9	
Incr. 8 to 12	(30.6)				16.9	13.9	14.7	16.6	18.5	18.4	13.2	
<i>Distal</i>												
Age 8	4.6	4.6	4.4	3.2	2.0	2.8	3.7	3.9	3.9	4.4	7.0	
Age 12	44.4	42.7	39.5	36.0	31.4	27.9	24.4	29.7	35.1	23.4	29.0	
Incr. 8 to 12	(39.8)				26.8	23.3	20.0	26.5	33.1	19.5	25.1	
<i>Buccal</i>												
Age 8	2.5	2.1	1.2	0.8	0.4	0.9	1.1	1.3	1.1	0.6	1.3	
Age 12	4.4	4.0	3.0	2.7	2.5	2.3	1.8	1.3	0.8	2.4	0.6	
Incr. 8 to 12	(1.9)				0	0.2	0.6	0.5	0.4	1.1	-0.5	
<i>Lingual</i>												
Age 8	1.5	0.9	0.5	0.3	0.3	0.7	0.7	0.8	0.6	0	0.4	
Age 12	4.7	4.7	4.6	4.0	3.5	3.2	2.4	2.0	2.0	2.4	2.3	
Incr. 8 to 12	(3.2)				2.0	2.3	1.9	1.7	1.7	1.6	1.7	
LOWER CENTRAL INCISORS <sup>1</sup>												
<i>Any Surf. (DF)</i>												
Age 8	5.8	5.8	3.5	1.8	1.2	1.0	0.9	0.8	0.9	0	0.8	
Age 12	19.8	19.2	16.9	15.2	11.6	8.7	5.8	3.7	3.0	0.6	2.8	
Incr. 8 to 12	(14.0)				5.8	2.9	2.3	1.9	1.8	-0.2	1.9	
<i>Mesial</i>												
Age 8	4.1	3.3	2.3	1.5	0.8	0.7	0.5	0.5	0.3	0	0.8	
Age 12	15.5	15.5	12.7	11.6	8.1	6.1	3.8	2.3	0.8	0.6	0.6	
Incr. 8 to 12	(11.4)				4.0	2.8	1.5	0.8	0	0.1	0.3	
<i>Distal</i>												
Age 8	2.1	2.1	1.4	0.7	0.5	0.4	0.4	0.4	0.5	0	0.4	
Age 12	13.8	12.8	11.4	10.0	7.7	5.5	3.6	2.3	1.0	0	2.3	
Incr. 8 to 12	(11.7)				5.6	3.4	2.2	1.6	0.5	-0.4	1.8	

<sup>1</sup> Buccal and lingual rates omitted.

( ) Expected increments, assuming no significant trend in age-specific rates from 1937 to 1941.

\* Reduced population, only 3 villages.

Table 10. Per cent of specific surfaces with caries for the permanent lateral incisors of 8- and 12-year-old children examined in the villages in 1940-1941 to 1948-1949 and in 1951-1952 and 1952-1953, and increments in per cents of surfaces carious between ages 8 and 12. Smoothed annual rates, 1941 to 1949.

TOOTH SURFACE AND AGE	SCHOOL YEAR										
	1941	1942	1943	1944	1945	1946	1947	1948	1949	1952*	1953*
UPPER LATERAL INCISOR											
<i>Any Surf. (DF)</i>											
Age 8	10.8	10.8	8.4	5.7	3.0	5.6	7.8	8.3	8.3	8.7	13.8
Age 12	55.9	51.7	48.8	45.5	43.1	37.7	32.3	38.0	43.8	30.5	39.9
Incr. 8 to 12	(45.1)				32.3	26.9	23.9	32.3	40.8	22.2	31.6
<i>Mesial</i>											
Age 8	7.6	7.6	6.1	4.1	2.0	4.4	6.1	6.7	6.7	6.7	10.1
Age 12	42.8	41.0	37.8	34.7	31.6	28.4	25.2	30.3	35.4	22.6	29.5
Incr. 8 to 12	(35.2)				24.0	20.8	19.1	26.2	33.4	15.9	22.8
<i>Distal</i>											
Age 8	0.6	0.6	0.8	0.4	0	0	0.7	0	1.0	0	0.6
Age 12	15.6	14.0	10.6	8.4	5.4	3.9	2.4	4.9	6.3	2.4	7.5
Incr. 8 to 12	(15.0)				4.8	3.3	1.6	4.5	6.3	2.4	6.5
<i>Buccal</i>											
Age 8	1.8	1.1	0.7	0.3	0	0.4	0.9	0.9	1.1	1.0	0
Age 12	3.4	3.4	3.0	2.4	1.9	1.4	0.9	1.9	2.4	1.8	1.2
Incr. 8 to 12	(1.6)				0.1	0.3	0.2	1.6	2.4	0.9	0.1
<i>Lingual</i>											
Age 8	3.5	2.7	1.9	1.5	1.1	1.5	1.5	1.4	1.4	1.9	3.8
Age 12	16.8	16.8	15.3	15.2	15.5	13.5	10.1	11.3	11.3	7.9	12.7
Incr. 8 to 12	(13.3)				12.0	10.8	8.2	9.8	10.2	6.5	11.3
LOWER LATERAL INCISOR <sup>1</sup>											
<i>Any Surf. (DF)</i>											
Age 8	4.2	3.2	2.3	1.2	0	0.5	0.5	0	0	0.7	0.5
Age 12	14.4	14.4	12.6	10.9	7.6	6.1	4.5	3.1	1.7	0.6	2.3
Incr. 8 to 12	(10.2)				3.4	2.9	2.2	1.9	1.7	0.6	2.3
<i>Mesial</i>											
Age 8	3.4	2.2	1.5	0.7	0	0.5	0.5	0	0	0.7	0.5
Age 12	12.9	12.8	11.4	9.7	6.9	5.1	3.4	2.3	1.2	0.6	1.7
Incr. 8 to 12	(9.5)				3.5	2.9	1.9	1.6	1.2	0.6	1.7
<i>Distal</i>											
Age 8	1.1	0.7	0.4	0.1	0	0	0	0	0	0	0
Age 12	6.0	4.7	3.5	2.5	1.3	1.1	1.1	0.7	0.3	0	1.1
Incr. 8 to 12	(4.9)				0.2	0.4	0.7	0.6	0.3	0	1.1

<sup>1</sup> Buccal and lingual rates omitted.

( ) Expected increments, assuming no significant trend in age-specific rates from 1937 to 1941.

\* Reduced population, only 3 villages.

specific surfaces of the permanent incisors of the 8- and 12-year-old children and increments of caries for successive quasi-cohorts are shown in Tables 9 and 10 for central and lateral

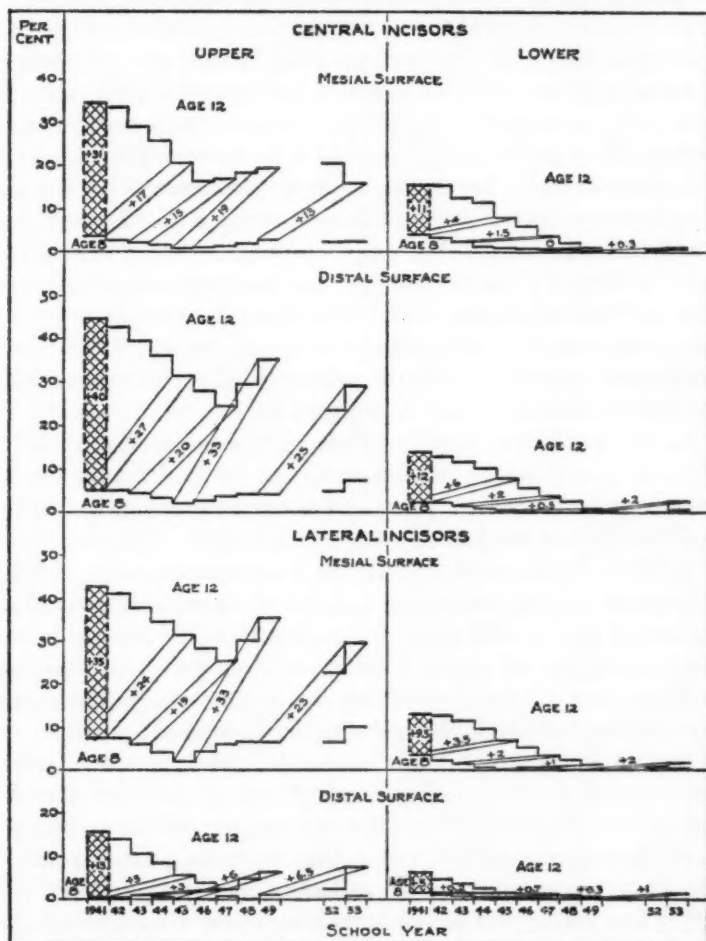


Fig. 9. Increments of caries in specific surfaces of incisors during selected four-year periods from age 8 to 12 years, and for 8- and 12-year-old children, smoothed annual DF rates for school years 1940-1941 to 1948-1949, and actual rates in 1951-1952 and 1952-1953 for a reduced population.

incisors, respectively. Trends in the annual rates at the two ages and increments from age 8 to 12 years for selected cohorts are shown in Fig. 9 only for mesial and distal surfaces.

Most of the caries in incisors occur in the mesial and distal surfaces with the exception of upper lateral incisors in which the lingual surface is carious more often than the distal. At age 8, these teeth are relatively free of caries, and in 1941 specific surface rates were less than 5 per cent except for the mesial surface of upper lateral incisors (7.6 per cent). Consequently the caries rates at age 12 were largely determined by the accumulation of caries during the age period 8 to 12 years and trends in rates for children aged 12 years are very nearly the same as trends in increments for the war and post-war years. The patterns of change in the specific-surface rates at age 12 are similar to those previously discussed for the 13-year-old group, and, therefore, only the principal indications concerning changes in increments are mentioned here.

In the mesial and distal surfaces of both central and lateral incisors, the caries accumulated by age 12 had begun to decrease in the early war years and rates were somewhat lower in 1941-1942 or 1942-1943 than in 1940-1941. For the cohort of children 8 years old in 1941, the increments of caries in the four years to 1945 were very much less than the expected or estimated prewar increment for each surface. However, in the lingual surface of upper lateral incisors, the prevalence of caries at age 12 remained fairly constant through 1945, and decreased relatively little in the next two years.

During the early post-war years, the trends in the specific-surface caries rates, and increments, were different for the upper and lower incisors. For the upper teeth, which are much more susceptible to caries than the lower, caries rates at age 12 decreased until 1947 and then increased in 1948 and 1949; but rates for the lower incisors show a continuous decrease to 1949 when only 3 per cent of the teeth were carious. In 1947, caries rates at age 12 for distal surfaces of upper central incisors and for adjacent mesial surfaces of lateral incisors



indicate four-year increments of 20 and 19 per cent respectively, as against expected increments of 40 and 35 per cent. A similar reduction is shown for caries in the mesial surface of upper central incisors, and the increment to 1947 was 15 per cent compared to an expected 31 per cent. Increases in increments for children 12 years of age in 1948 and 1949 occurred in mesial and distal surfaces of both upper incisors. In the adjacent surfaces of upper lateral and central incisors, the caries increments had risen to 33 per cent in 1949; but for the mesial surface of central incisors and distal surface of lateral incisors, only small increases in the increments are shown. The increase for the lingual surface of upper lateral incisors also was very small.

In the two school years, 1951-1952 and 1952-1953, caries rates for the incisors show wide annual fluctuations, although examinations were made in the same three villages. However, the number of children examined each year was small and none of the differences between rates for the two years is statistically significant; that is, all observed differences had more than a five per cent chance of occurring on the basis of sampling alone. Therefore, an average rate for the two years may be considered representative of the prevalence of caries in this period. For both lower incisors, caries rates remained negligible at age 12. For upper incisors, at 8 years of age the DF tooth rates were somewhat higher than in 1948 and 1949. In the central incisors, a small increase is indicated only for the distal surface; but in the upper lateral incisors, rates for both the mesial and lingual surfaces were higher. For the three surfaces which show an increase at age 8, the caries prevalence in the later period was similar to that recorded in 1940 and 1941. For the children 12 years of age in 1952 and 1953, there is no indication that the prevalence of caries in any of the incisors had increased to levels above those noted for 1949 and both the rates and four-year increments continued to be less than those shown for 1940-1941.

The pattern of time changes in caries prevalence shown for

upper incisors of the 12-year-old children after the war differs from that previously described for the first molars. Whereas the DF rates for mesial and distal surfaces of upper incisors, and also the DF tooth rates, were at their lowest levels in the school year 1946-1947, increased in the next two years, and apparently leveled off in the later period, the corresponding rates for upper and lower first molars continued to decrease in 1948, remained near the same level in 1949, and then in the later period increased sharply. The total accumulation of caries in the occlusal surfaces of the molars at age 12 also decreased until the year 1949, and the incidence of caries-free surfaces between age 8 and 12 years was less for the period 1945 to 1949 than in any previous four-year period. This delay in the increase in caries in first molars as compared with the upper incisors most probably is the result of differences in mouth-exposure time during the war. The average age at eruption for first molars is 6 years, for upper central incisors 7 years and for upper lateral incisors 8 years. Thus, in 12-year-old children examined in 1948, first molars erupted in 1942, central incisors erupted in 1943, and lateral incisors erupted in 1944. In a previous report (Tøverud, 1956) evidence of delayed eruption of most of the permanent teeth was found for the years after the three-year period 1941-1943. The upper incisors showed great delay in eruption in the period 1944-1946; and for these teeth, about one-half year may be added to the eruption time. Consequently, the permanent first molars of children aged 12 in 1948 or 1949 had a much longer mouth-exposure time than the incisors during the war years when the intake of easily fermentable carbohydrates was low and oral conditions were favorable for maturation.

#### GENERAL DISCUSSION

This report on caries in specific surfaces of permanent teeth of village school children in Norway has been concerned with three general questions: (1) occurrence of caries in specific surfaces of each tooth at an early post-eruptive time and, for

first molars and incisors, after longer mouth exposure; (2) trends in specific surface rates for children at selected ages during the war and post-war years; and (3) changes in increments of caries in first molars and incisors between age 8 and 12 years for successive groups as a method of evaluating the effect of war and post-war conditions on caries prevalence.

In his studies on "The Occurrence of Dental Caries in the Permanent Dentition" Welander (1955) concludes: "Proximal surfaces on the same tooth tend to have the same caries status," and "The abutting teeth tend to have the same caries status." The population studied by Welander consisted of conscripts. It would seem natural that these conditions change with age and the caries experience of these 13-year-old children shows many expectations. From Fig. 3 it is clearly seen that the caries rates for the two proximal surfaces of a tooth may differ very much; note differences in the first molar, upper lateral incisor, upper first bicuspid and upper central incisor. Fig. 3 also illustrates the great tendency for contacting surfaces to have corresponding caries rates. Thus, in the upper jaw, rates are similar for: distal surface of central incisor and mesial surface of lateral incisor; distal surface of lateral incisor and mesial surface of cuspid; distal surface of cuspid and mesial surface of first bicuspid. Differences in the caries status of abutting teeth in this age group are shown for the cuspids which differ very much from their neighbors, the second bicuspid which differs from the first molars, and the lower second bicuspid which differs from the lower first bicuspid.

The rank of the specific teeth according to their DMF tooth rates also will differ with age during childhood and adolescence. Thus, the rank of the DMF tooth rates for these 13-year-old children does not conform with that of adults.

In an earlier report (Toverud, 1957 II and III) dealing with the changes in DMF tooth rates and DMF total surface rates for the permanent first molars it was shown that the younger teeth reacted earlier than the older teeth both as to decrease and to increase. This difference is demonstrated by trends

shown in Figs. 1 and 3. When specific surface rates are examined, it is found that changes in caries rates differed not only from tooth to tooth, but also for surfaces in the same tooth. In young teeth, some change in caries rates usually became evident in all the surfaces soon after conditions affecting dentition occurred, and the differences among surfaces of the same tooth were largely those of degree. The most susceptible surfaces in which the caries prevalence in 1940-1941 was at a high level at an early tooth age had the largest absolute reduction in caries during the war years and usually had an increase early in the post-war period. Surfaces in which the caries rate was low in the earliest post-eruptive years had smaller absolute reductions in caries and tended to have a more delayed increase in the post-war period. The larger absolute reductions in the high surface caries rates represent smaller percentage changes than the percentage changes in the lower surface caries rates, but the high absolute changes have more practical significance.

Trends in rates for specific surfaces of the first molars of 7-year-old children shown in Fig. 1 illustrate the differences. Both in the upper and lower molars, the caries rate of the fissure-pit surfaces, especially the occlusal surfaces, not only decreased rapidly during the war years but also increased very soon after the war. For the smooth surfaces, including mesial and distal surfaces of upper and lower molars, buccal in the upper and lingual in the lower molar, caries rates decreased in each of the war years but increased less quickly after the war. In the second molars and in the bicuspid of 13-year-old children (young teeth), the fissure-pit and the smooth surfaces tend to exhibit changes in caries rates similar to those noted for first molars of the 7-year-old children.

For the first permanent molars of 12- and 13-year-old children, there were very large differences among trends of specific surface caries rates, and for most surfaces the trends differed sharply from the DMF tooth trends. For these teeth with a post-eruptive age of about 7 years in 13-year-old children, the

DMF tooth rates and also the occlusal surface rates shown in Fig. 3 were about 95 per cent or more until 1948 and 1949 when a small decrease occurred for the teeth which had erupted after the beginning of the war. For some other surfaces, however, the decreases in caries rates were much greater and began early in the period. For the mesial and distal surfaces, caries rates initially were so high that the reduction demonstrated is of great significance to the preservation of this tooth.

In his interesting publication "Epidemiological Trend Patterns of Dental Caries Prevalence Data," Knutson (1958) has demonstrated on large groups of children from different places in the USA a high correlation between the percentage of children with one or more DMF teeth,<sup>9</sup> rates for DMF teeth and DMF surfaces. Baume (1959) in his publication "Evaluation of Caries Preventive Procedures" states: "The DMF tooth rate per person is the appropriate index for estimating past and present caries experience (prevalence) of a population of some magnitude. Prevalence within smaller samples may be expressed by the DMF surface rate per person."

This analysis of trends in caries rates has shown that DF surface rates are a more sensitive measure of change than the DF tooth rates. When caries in a single susceptible surface, such as the occlusal surface, dominates the tooth rate, only changes in this surface are measured by the DF tooth rate; and, when the caries rate for one surface is high at the time conditions change, benefits resulting from less caries activity in other surfaces are not revealed by the tooth rate. Thus, a record of surface caries is important for evaluating effects of prospective caries preventive methods.

<sup>9</sup> The presence or absence of caries in the first permanent molars will almost always determine the percentage of children with one or more DMF teeth, since these teeth are the first to erupt and are very susceptible to caries. Variations in this percentage for a specific age group will reflect the caries producing conditions affecting the group if conditions have been constant. However, a change in conditions can modify this percentage only to the extent that it can affect the incidence in caries-free children; and, consequently, if first molars have a high prevalence of caries soon after eruption, as in the Norwegian children before the War, only the percentage for younger children will be modified by new factors affecting caries, that is, the percentage for children with newly erupted first molars.

During years of changing oral conditions, increments of caries acquired within definite periods of time can measure more directly the effects of new conditions on caries activity in older teeth than the rates for all accumulated caries which are affected in varying degree by mouth exposure under different conditions at different post-eruptive periods. In this study of increments of caries in the incisors during the age period 8 to 12 years, the increments followed closely the total DF rates at age 12, since the caries prevalence accumulated to age 8 is very little. However, for the first molars, especially the occlusal surface, the four-year increments for the children 12 years of age in 1947 to 1949 were increasing, but the DF prevalence rates continued to decrease or remained constant because at age 8 these children had many fewer caries attacks in the first molars than those examined in earlier years. Thus, the changes in increments of caries in first molars of the older school children describe more accurately the patterns of change in caries activity than do the annual DF rates. The timing of the effects of rapid changes in war and post-war conditions on the dentition would have been more clearly defined if increments for shorter periods than four years had been available.

It would be of interest to compare the differences in reduction of caries in specific teeth and specific surfaces found in this study with corresponding reductions after use of fluorides as a caries prophylactic measure. Unfortunately, the data are not directly comparable with that reported from any studies on the effects of fluorine.

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## AGE HEAPING IN THE UNITED STATES CENSUS: 1880-1950\*

MELVIN ZELNIK

ALTHOUGH the problem of age misreporting in the census (and other social surveys) has long been recognized,<sup>1</sup> few techniques have been developed for estimating and correcting the errors involved. Those techniques which are available for demographic analysis are, for the most part, concerned with digital preferences or five year age distributions;<sup>2</sup> as such they are inadequate for determining preferences or avoidances for individual years of age.

The method employed in this paper is an attempt to establish the size of error for each year of age, relative to its neighbors, for the native white male and native white female populations enumerated in the United States censuses from 1880 to 1950.<sup>3</sup> A concept of central importance is that of age ratio.

\* The material presented in this paper represents a revised version of a chapter in the author's unpublished doctoral dissertation, "Estimates of Annual Births and Birth Rates for the White Population of the United States from 1855 to 1934," Princeton University, 1959. The dissertation, along with other derivative results not included in it, is currently being prepared for publication.

<sup>1</sup> See Young, Allyn A.: Age, *Supplementary Analysis and Derivative Tables, Twelfth Census of the United States*, U. S. Bureau of the Census, 1900, pp. 130-174. In the remainder of the paper the term age heaping will be used rather than age misreporting. By age heaping the author means the recognized phenomenon of people reporting themselves at an age other than, but close to, their true age, as for example, the preference for ages ending in 0 and 5. Defined in this manner, age heaping can be considered a major type of age misreporting.

<sup>2</sup> Department of Social Affairs, Population Branch, United Nations, "Accuracy Tests for Census Age Distributions Tabulated in Five-Year and Ten-Year Groups," *Population Bulletin*, No. 2, October 1952: pp. 59-79; and "The Accuracy of Quality of Basic Data for Population Estimates, Chapter 3 in *Methods of Appraisal of Quality of Basic Data for Population Estimates*, ST/SOA, Series A, (Population Studies) No. 23.

<sup>3</sup> U. S. Census Office, 1880: *Statistics of the Population of the United States*, Table 20, pp. 548-550; *idem*, 1890, *Report on Population of the United States*, Part II, Table 1, pp. 2-5; *idem*, 1900, *Population*, Part II, Table 1, pp. 2-5; U. S. Bureau of the Census: *Population, General Report and Analysis*, Table 29, in *1910 Census of Population*, Vol. I, pp. 310-313; *idem*, *Population, General Report and Analytical Tables*, Table 9, in *1920 Census of Population*, Vol. II, pp. 162-165; *idem*, *General Report, Statistics by Subjects*, Table 21, in *1930 Census of Population*, Vol. II, pp. 595-596; *idem*, *Characteristics by Age*, Part I (U. S. Sum-

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As used in this paper, age ratio is defined as the ratio resulting from the number of persons at any age divided by the average of the ten adjacent ages, five on each side.<sup>4</sup>

It is important to make explicit the fact that no attempt is here being made to determine the amount or degree of under (or over) enumeration of the entire population or at any age.<sup>5</sup> Even a census of complete enumeration (and therefore no under or over enumeration at any age) could contain errors in the age distribution due to the preference of people for certain ages and the avoidance of others. The problem may be rephrased in this manner: errors in the number of people reported at any age in a census are composed of two parts, 1) the error resulting from the under or over enumeration of that age and 2) the error resulting from the tendency of people to avoid or select certain ages other than their "true" age. The method developed in this paper is concerned only with the second source of error.<sup>6</sup>

An age distribution of a closed population enumerated at any point in time is a consequence of four factors: 1) the number of births in each year preceding the count; 2) the number of deaths in each birth cohort from time of birth to time of

mary), Table 3, in *1940 Census of Population*, Vol. IV, p. 13; *idem*, *Characteristics of the Population, Part I (U. S. Summary)*, Table 94, in *1950 Census of Population*, Vol. II, pp. 1-165.

The study is restricted to the white population because the method used was found to be inapplicable to the nonwhites (see footnote 16). 1880 was picked as the starting point because of the poor quality of the 1870 census and because censuses prior to that date do not contain the necessary single year age distributions.

<sup>4</sup> This term has been introduced and defined at this point to eliminate the possibilities of confusion resulting from the use of a similar term that frequently has another meaning.

<sup>5</sup> See Coale, Ansley J., *The Population of the United States in 1950 Classified by Age, Sex, and Color—A Revision of Census Figures*. *Journal of the American Statistical Association*, March 1955, Vol. 50, No. 269: pp. 16-54.

<sup>6</sup> This is to some degree an overstatement. The method being used is designed to discover relative error but it is impossible to distinguish its source. A low age ratio may result from an under enumeration of that age or from an avoidance of that age. Under enumeration, however, covers a range of ages and to this extent its influence is mitigated by the technique being used.

This method is also inadequate for the detection of gross errors in age misreporting, i.e., when the selected age is not close to the true age. Some of this would undoubtedly be detected but cannot be adequately corrected for, as the method is based on the assumption that avoided or preferred ages are adjacent or close to the true age (see footnote 1). It seems unlikely that any noticeable amount of misreporting would be the result of this form of age preference.

enumeration; 3) age heaping; and 4) under and/or over enumeration of the population. (As previously mentioned, the fourth factor is outside the scope of the method being employed.)<sup>7</sup> It follows that age ratios calculated on the basis of a given age distribution will also be affected, in varying degrees, by these factors. If the provisional assumption is made that, over a "short span of time" the number of births does not deviate from a linear trend and that deaths do not cause a marked deviation from linearity, the age ratios will approximate unity; where deviations do result, they will be the result of age heaping.<sup>8</sup>

On the basis of this rationale, age ratios were calculated for the native white males and native white females enumerated in each of the eight censuses from 1880 to 1950.<sup>9</sup> A ten year age-interval was used in the calculation of these ratios because: 1) it was considered short enough to approximate a straight line; 2) long enough to reduce the effect of small yearly fluctuations from the trend; and 3) the denominator for each age was composed of a series of ages ending in all other digits.<sup>10</sup>

The effect of mortality on the age ratios was estimated by the use of life tables. Age ratios were calculated for the "populations"<sup>11</sup> of different life tables widely separated in time.<sup>12</sup> In

<sup>7</sup> A closed population is here being approximated as the techniques are being applied to the native white populations. Emigration is of negligible importance except for the males in the 1950 census, owing to the size of the military forces overseas. In the hope of increasing the readability of this paper, technical details have been put in appendices at the end. For the way in which the 1950 overseas population was reallocated, and adjustments made in the 1930 and 1940 figures to provide necessary single year age distributions of comparable populations, see Appendix A.

<sup>8</sup> The assumption of the linearity of births and the effects of mortality will be examined and corrected for in subsequent sections of the paper. In addition, see Appendix B.

<sup>9</sup> See footnote 3 and Appendix A.

<sup>10</sup> More accurately, the denominator for each age was composed of a series of ages in which eight digits appeared once and one digit twice. See Appendix C for the manner in which adjustments were made to age ratios affected by the duplication of a digit.

<sup>11</sup> i.e., the  $L_x$  columns of the life tables.

<sup>12</sup> For the females: U. S. Bureau of the Census, *United States Life Tables 1890, 1901, 1910 and 1901-1910*, prepared by Glover, James A., Table 21, pp. 92-93; *idem*, *United States Life Tables and Actuarial Tables 1939-1941*, prepared by Greville, Thomas N. E., Table 6, pp. 36-37.

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each series deviations from unity were of negligible proportions in the years of low mortality (i.e., ages 10-50); more noticeable deviations appeared at the earlier and later ages.

These life table age ratios were taken as evidence of the need for adjustment of the population age ratios in so far as they indicated the effect of mortality on the linearity of the enumerated populations. The same correction factor was applied to all population age ratios for a specific age, as calculated from each of the eight censuses. This seemed justifiable in view of the small differences existing between the two series of life table age ratios for the same year of age. The single correction, for each age, was arrived at by averaging the life table age ratios for corresponding years of age. Where this average deviated from unity by less than .005, no correction was applied; where the average deviated from unity by .005 or more, the population age ratios were corrected by dividing through by this factor. Corrections were of minor influence except at the extreme ages. The native white female life table adjusted age ratios, and the correction factor applied to each age, are shown in Table 1.<sup>13</sup>

The ideal situation for estimating the validity of the assumption of linearity of births (and for correcting where invalid) would be an annual series of birth statistics for the total United States beginning about 1850. Unfortunately, these figures are lacking, birth statistics for the total United States not having been collected until 1933, when the birth registration area first included all states.<sup>14</sup> The method that has been used to correct for the provisional assumption of the linearity of births depends instead on the identification of large and

For the males: *United States Life Tables 1890, 1901, 1910 and 1901-1910*, Table 19, pp. 90-91 and *United States Life Tables and Actuarial Tables 1939-1941*, Table 5, pp. 34-35.

<sup>13</sup> Although both sexes were treated separately, but by identical methods, tables and discussion are restricted mainly to the females. Final results are shown for the males.

<sup>14</sup> An attempt was made to use the birth statistics available for a few states as far back as the mid-1800's. For the manner in which these figures were handled and the reasons why an alternative method proved necessary, see Appendix D.

Table 1. Age ratios\* for census enumerated native white females, ages 5-85, 1880-1950, adjusted by life table correction factor.

AGE	CENSUS								LIFE TABLE CORRECTION FACTOR
	1880	1890	1900	1910	1920	1930	1940	1950	
5	1.041	1.047	1.019	1.013	1.037	1.049	1.006	.943	.992
6	1.062	1.093	1.043	1.025	1.026	1.035	.936	.989	1.000
7	.994	.988	1.020	.983	1.005	1.004	.951	1.059	1.000
8	1.016	1.007	1.031	.980	1.013	1.068	.997	.986	1.000
9	.955	.932	.979	.947	.973	1.025	.983	.951	1.000
10	1.057	1.012	1.032	.986	1.022	1.011	1.020	.953	1.000
11	.939	.894	.968	.928	.995	.968	.979	.945	1.000
12	1.083	1.051	.990	1.029	1.057	1.028	1.041	.992	1.000
13	.976	.949	.965	.976	.985	.978	1.010	.973	1.000
14	.969	1.032	.990	1.022	1.002	1.010	1.007	.956	1.000
15	.847	.968	.995	.967	.938	.982	1.006	.962	1.000
16	.933	1.077	1.030	1.060	1.017	1.023	1.031	.930	1.000
17	.916	.962	.992	.998	.963	.990	.987	.945	1.000
18	1.156	1.116	1.022	1.089	1.009	1.043	1.086	1.002	1.000
19	1.018	.943	.967	.967	.971	.995	1.025	1.004	1.000
20	1.146	1.072	1.021	1.035	.969	1.008	1.000	1.002	1.000
21	.961	.958	.946	.963	.963	1.004	1.008	1.015	1.000
22	1.102	1.070	1.027	1.012	1.017	1.030	.988	.999	1.000
23	1.012	1.008	1.017	1.008	1.028	1.006	.992	.996	1.000
24	1.028	.989	1.052	1.022	1.040	1.009	1.010	1.001	1.000
25	1.065	.984	1.062	1.053	1.056	1.013	1.025	1.065	1.000
26	.961	.911	.981	1.012	1.026	.985	1.009	1.025	1.000
27	.866	.877	.984	.931	.981	.932	.938	.994	1.000
28	1.040	1.102	1.015	1.061	1.055	.987	1.028	1.052	1.000
29	.814	.896	.934	.893	.930	.985	.995	1.012	1.000
30	1.310	1.358	1.155	1.158	1.135	1.127	1.078	1.064	1.000
31	.711	.786	.875	.798	.856	.846	.902	.943	1.000
32	.992	1.038	1.008	1.033	1.010	1.023	1.064	1.020	1.000
33	.903	.947	.959	.925	.926	.945	.965	.970	1.000
34	.896	.928	.936	.984	.959	.978	1.000	.993	1.000
35	1.227	1.138	.985	1.116	1.111	1.096	1.036	1.052	1.000
36	1.004	.979	.893	.995	1.000	1.010	.970	1.010	1.000
37	.888	.880	.919	.926	.911	.960	.917	.980	1.000
38	1.074	1.067	1.104	1.121	1.105	1.115	1.000	1.031	1.000
39	.886	.868	1.071	.952	.972	.961	1.033	.998	1.000
40	1.436	1.350	1.207	1.251	1.175	1.174	1.148	1.108	1.000
41	.684	.731	.886	.789	.779	.815	.833	.907	1.000
42	.999	1.008	1.030	1.081	1.079	1.079	1.073	1.088	1.000
43	.882	.891	.954	.920	.947	.924	.961	.987	1.000
44	.920	.895	.970	.877	.928	.921	.941	.931	1.000
45	1.233	1.239	1.070	1.043	1.137	1.113	1.060	1.023	1.000
46	.904	.951	.908	.830	.909	.934	.964	.939	1.000
47	.890	.896	.932	.886	.946	.924	.993	.939	1.000
48	1.023	1.047	.989	1.134	1.053	1.064	1.063	.976	1.000
49	.873	.861	.972	1.040	.987	.965	.970	1.057	1.000
50	1.492	1.459	1.204	1.335	1.293	1.230	1.175	1.163	1.000
51	.714	.728	.880	.825	.813	.795	.864	.859	1.000
52	1.039	1.012	.991	1.078	1.064	1.051	1.053	1.025	1.000
53	.901	.918	.920	.925	.928	.946	.932	.963	1.000
54	.982	1.013	.980	1.014	.943	1.039	1.005	1.008	1.000



Table 1 (Continued)

AGE	CENSUS								LIFE TABLE CORRECTION FACTOR
	1880	1890	1900	1910	1920	1930	1940	1950	
55	1.103	1.042	1.081	1.020	.934	1.035	1.010	1.002	1.000
56	1.000	1.034	1.000	.972	.905	.962	.975	.987	1.000
57	.815	.881	.933	.865	.885	.925	.931	.996	1.000
58	.967	.964	.948	.984	1.089	1.034	1.046	1.039	1.005
59	.841	.782	.959	.912	1.015	.953	.973	.975	1.005
60	1.512	1.436	1.193	1.241	1.282	1.250	1.124	1.103	1.006
61	.721	.705	.819	.767	.816	.798	.808	.872	1.007
62	1.006	.970	.975	.986	1.019	1.012	.967	.963	1.008
63	.955	1.013	1.012	.976	1.028	.993	.986	.957	1.008
64	.936	.971	.985	.967	.957	.918	1.017	.958	1.009
65	1.213	1.240	1.064	1.196	1.136	1.088	1.202	1.211	1.009
66	.886	.918	.978	.924	.860	.798	.904	.952	1.010
67	.880	.898	.967	.917	.884	.878	.966	.966	1.010
68	1.007	.970	.932	.998	.966	1.076	1.005	.999	1.011
69	.886	.803	.932	.938	.956	1.045	.968	.940	1.011
70	1.332	1.352	1.112	1.138	1.122	1.191	1.138	1.065	1.012
71	.728	.744	.816	.754	.791	.844	.842	.841	1.012
72	.990	1.019	1.000	1.019	1.002	1.047	1.020	1.012	1.012
73	.943	.946	.943	.958	.931	.960	.966	.988	1.012
74	.958	.944	.973	.965	.960	.945	.914	.999	1.011
75	1.120	1.108	1.080	1.074	1.155	1.076	.975	1.085	1.009
76	.972	.948	.941	.999	.960	.925	.863	.960	1.006
77	.846	.871	.929	.897	.902	.852	.856	.913	1.000
78	.931	1.017	.938	.942	.935	.920	1.037	.980	1.000
79	.906	.815	.926	.883	.950	.916	1.037	.949	.991
80	1.340	1.228	1.092	1.037	1.005	1.044	1.126	1.106	.982
81	.712	.792	.887	.773	.790	.824	.897	.880	.972
82	.917	.972	.950	.986	.976	.960	.995	.999	.960
83	.897	.970	.924	.969	1.005	.950	.983	.989	.946
84	1.068	1.084	.976	1.048	1.024	.996	.985	.971	.930
85	.954	1.004	.923	.976	.944	.979	.932	.895	.912
Average Deviation	.118	.106	.058	.076	.070	.066	.051	.045	

$$\text{* Age Ratio} = \frac{\text{Number at any age}}{\text{Average of ten adjacent ages}}$$

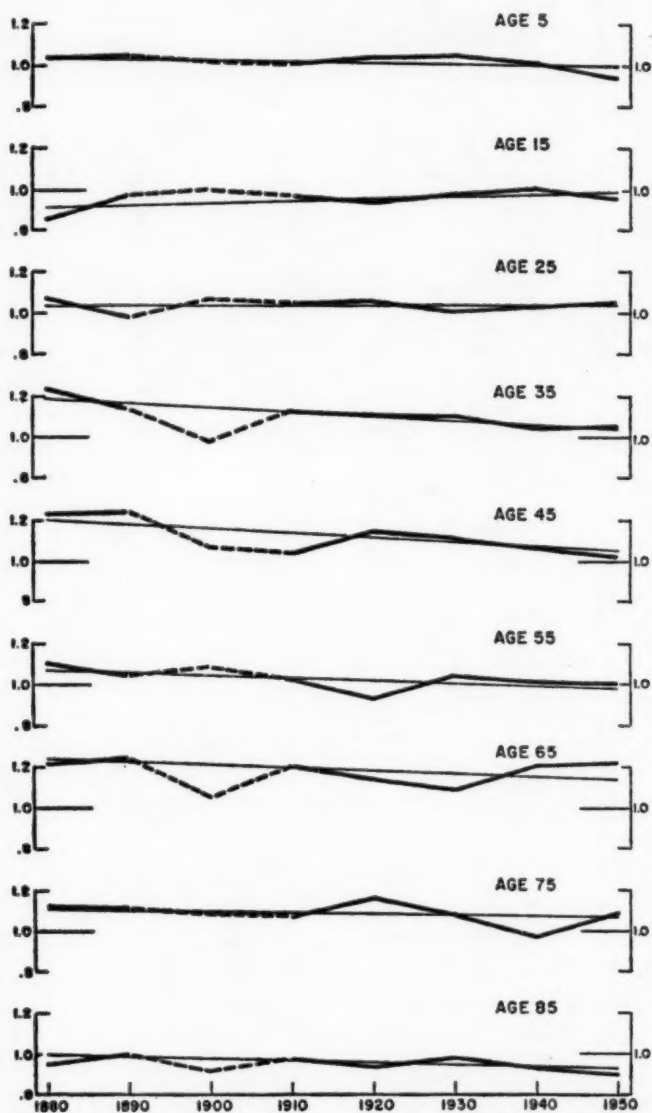
Sources: The age ratios have been calculated from the United States censuses 1880-1950 (see footnote 3). The life table adjustment factor is the average of the life table age ratios calculated from the  $1L_x$  values of the 1901 and 1939-1941 life tables (see footnote 12).

small birth cohorts (relative to their neighbors) at points in time some years after birth. More explicitly, cohort size was estimated from its effect on the age ratios calculated from the enumerated populations in a series of censuses.

Table 1 shows a decrease in the average deviation (the mean of the absolute deviations from unity) for each census, if 1900 is ignored.<sup>15</sup> When the age ratios for each year of age are

<sup>15</sup> This is true of the males also, where the values are 1880: .107; 1890: .097; (Continued on page 548)

# AGE RATIOS & FIRST TREND LINE



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Fig. 1. Trend lines fitted to age ratios of enumerated native white females, censuses of 1880-1950: ages ending in 5. Source: Table 1.

# AGE RATIOS & FIRST TREND LINE

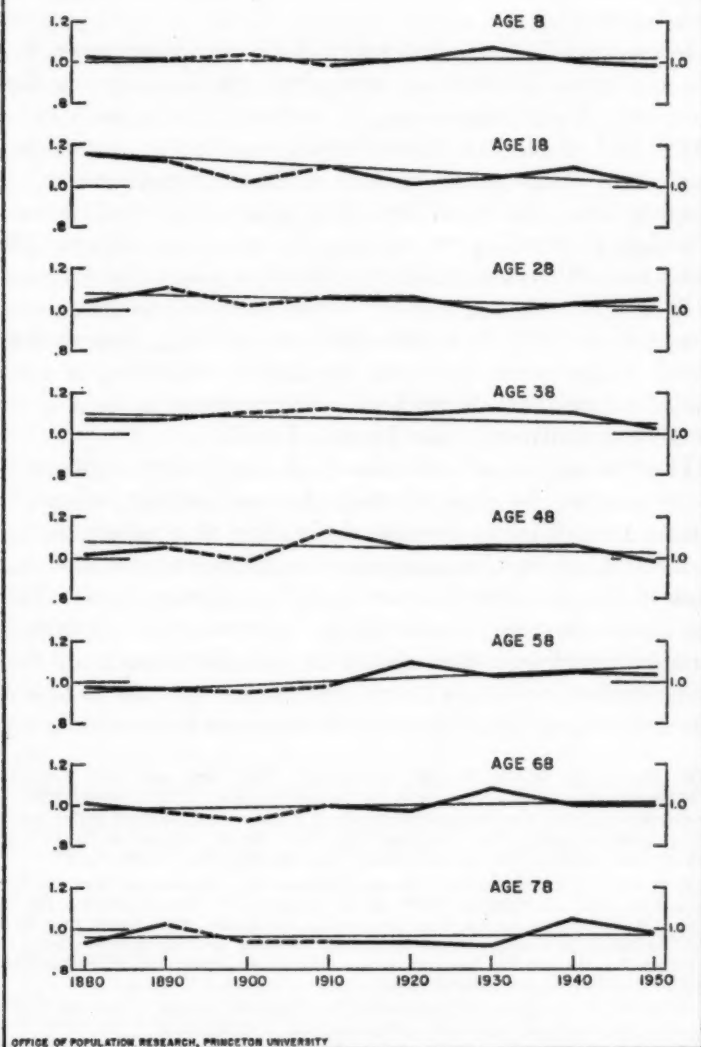


Fig. 2. Trend lines fitted to age ratios of enumerated native white females, censuses of 1880-1950: ages ending in 8. Source: Table 1.

plotted, they appear to be approaching unity in a linear fashion,<sup>16</sup> with the exception in most instances of 1900 (see Figures 1 and 2).<sup>17</sup>

It was therefore assumed, on the basis of this evidence, that the age ratios for each age were, through time, approaching unity in a linear fashion (again, with the noted exception of 1900) with deviations from a trend line due to the original size of the birth cohort and to random fluctuations of age heaping about the trend line. For each age a trend line was established by taking the average, for one point, of the 1880, 1890, and 1910 values and for the other point the values of 1920, 1930, 1940, and 1950.<sup>18</sup> These average age ratios were centered on 1893 1/3 and 1935 respectively; intermediate censal values were estimated by simple interpolation while values for censuses beyond these points were arrived at by simple extrapolation. (See Figures 1 and 2).

The true size of a birth cohort relative to its neighbors remains very nearly constant through time (except perhaps for certain ages of males through decimation of a cohort by war losses). A cohort of unusual size should appear as a deviation from the trend line each time the cohort is enumerated. Thus the birth cohort of 1864 can easily be traced through time by its appearance as a deviation from eight separate trend lines (representing eight ages) from 1880 when it was age 15 to 1940 when it was age 75 (Figure 1). If the trend lines actually rep-

1900: .054; 1910: .066; 1920: .061; 1930: .057; 1940: .046; and 1950: .038. The 1900 census asked not only "age at last birthday" but also "date of birth," the only time (prior to 1960) this information has been requested in a United States census. All available evidence seems to suggest a higher degree of accuracy in the 1900 age distributions resulting from the inclusion of this question. See Young, *op. cit.*

<sup>16</sup> It was at this point that it became apparent that the method used to adjust the native white populations would not be applicable to the nonwhites. The life table adjusted age ratios for them are clearly curvilinear; this meant that much more sophisticated methods would be needed, assuming that the problem was even solveable. No attempt has been made to work out an alternative technique which would be applicable to the nonwhites.

<sup>17</sup> To avoid repetition of figures which are basically similar, it was decided to include figures for some ages only, for the purpose of illustration.

<sup>18</sup> Since 1900 obviously does not fit the trend of age heaping, it would be incorrect to include it for the establishment of the trend line. This also resulted in treating the figures for this year slightly differently; see below.

resent the degree of age heaping, the deviations from the trend lines reflect the size of the birth cohorts. The size of the deviations does not remain constant because of random elements and consequently the average of the deviations was considered to be the closest approximation to the size of the birth cohort.<sup>19</sup>

The deviations of each birth cohort from the trend lines as it moves through time, and is therefore enumerated at an age approximately 10 years older in each census,<sup>20</sup> were totaled and averaged; this average figure may be considered the deviation of the age ratio from the trend line, due to cohort size. Because of the non-consistency of the 1900 census with the general assumption of linearity in age heaping, the deviations of the cohorts enumerated in 1900 were not included in estimating the average size of the cohort.

The trend lines themselves, however, were influenced by the size of the cohort. In other words, the age ratios used to determine the trend lines reflect both age heaping and the relative size of the birth cohort. An estimate of cohort size based on these trend lines will not be a true measure of actual cohort size but may be considered a first approximation to it. Consequently, a correction factor was derived, reflecting the influence of the cohort on the trend lines, and applied to the average cohort deviation.<sup>21</sup> This corrected cohort deviation (which is the first approximation to the relative size of the birth cohort) was used to adjust the age ratios, taking into account the differences in census date.

Since the age ratios have been corrected by the size of the cohort (or at least a first approximation to it), they may be considered a "more pure" measure of age heaping, and trend lines based on these corrected age ratios a more accurate rep-

<sup>19</sup> To the degree that random fluctuations are actually present, they tend to cancel one another.

<sup>20</sup> A major source of complication arises from the fact that the censuses have not always been taken as of the same date. This means that we are not dealing with the same birth cohort as it moves through time and is enumerated at each census. For the differences in census dates and the way in which this problem was handled, see Appendix E.

<sup>21</sup> See Appendix F for the derivation of this correction factor.

resentation of the linear trend in this phenomenon. This also implies that the differences between the original age ratios (as corrected for mortality) and trend lines established on the basis of the cohort adjusted age ratios will provide a more accurate measure of the size of the cohort.

Following this line of reasoning, trend lines were calculated from the cohort adjusted age ratios in the same manner as the first trend lines.<sup>22</sup> These trend lines, following the original assumption of linearity in age heaping, are the degree of age heaping for each year of age at each census, with the exception of 1900.<sup>23</sup>

If this assumption is valid, then the age ratios, corrected by adjustment for cohort size from the second approximation trend lines, should agree very closely with the trend line values. The "closeness of fit" between these two measures is shown in Figures 3 and 4. While not perfect, it can be seen that there is a high degree of improvement (see Figures 1 and 2) and a relatively good agreement.<sup>24</sup>

Theoretically the relative size of each birth cohort should be the same for both sexes, i.e., the correlation between the relative size of birth cohorts as estimated for males and females should be 1.00. A scatter diagram of these two series (Figure 5) shows that the correlation, while not unity, is extremely high and when calculated turns out to be 0.97. The deviations present can possibly be attributed to random elements present in age heaping, to the effects of under enumeration of one sex as compared to the other, or to the effects of war losses on the males.

Figure 6 shows these estimates of the relative size of the birth cohorts plotted against time. This diagram illustrates mainly

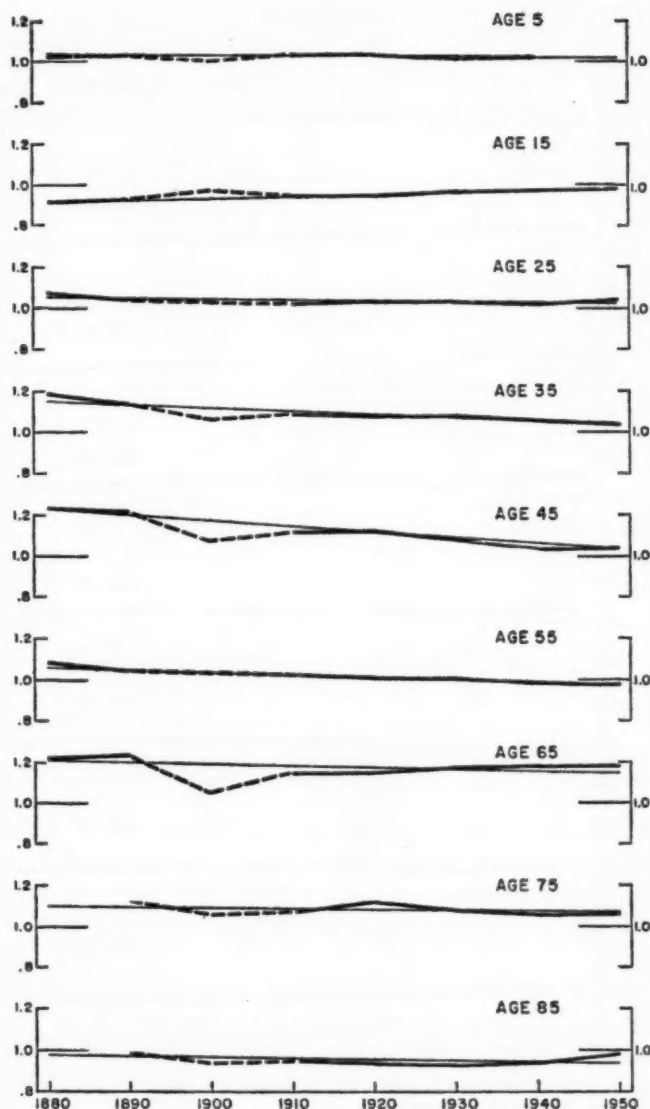
<sup>22</sup> Since every cohort had to be enumerated at least twice to estimate its size, it was not possible to adjust the age ratios for ages 5-14 in 1950 and 75-85 in 1880. The second trend lines for ages 5-14 were thus arrived at by averaging the 1920, 1930, and 1940 figures, centering it on 1930; for ages 75-85, the left side of the trend line was the average of 1890 and 1910, centered on 1900. For all other ages, the procedure used was the same as in determining the first trend lines.

<sup>23</sup> See Appendix B.

<sup>24</sup> See Appendix G for discussion of ages 62-68, where it has been suggested a change has taken place in age heaping as a result of social security legislation.



# AGE RATIOS & SECOND TREND LINE

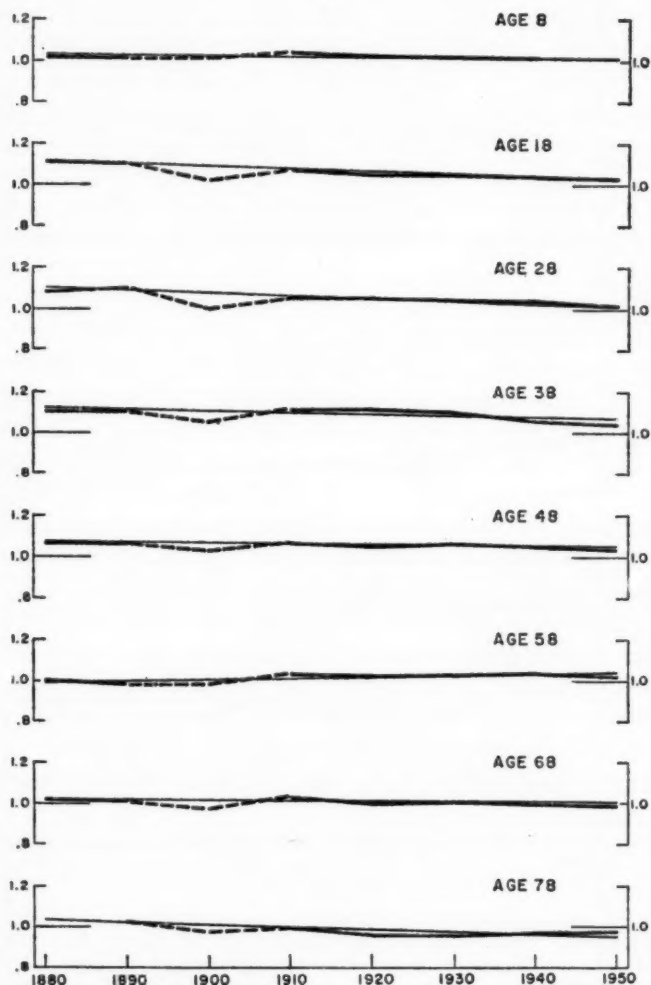


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Fig. 3. Modified trend lines and age ratios\* adjusted for cohort size, native white females, 1880-1950: ages ending in 5.

\* The trend lines have been fitted to the age ratios after the first correction for cohort size, while the age ratios shown are based on the second correction for cohort size; see text, p. 550.

# AGE RATIOS & SECOND TREND LINE



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Fig. 4. Modified trend lines and age ratios\* adjusted for cohort size, native white females, 1880-1950: ages ending in 8.

\* The trend lines have been fitted to the age ratios after the first correction for cohort size, while the age ratios shown are based on the second correction for cohort size; see text, p. 550.

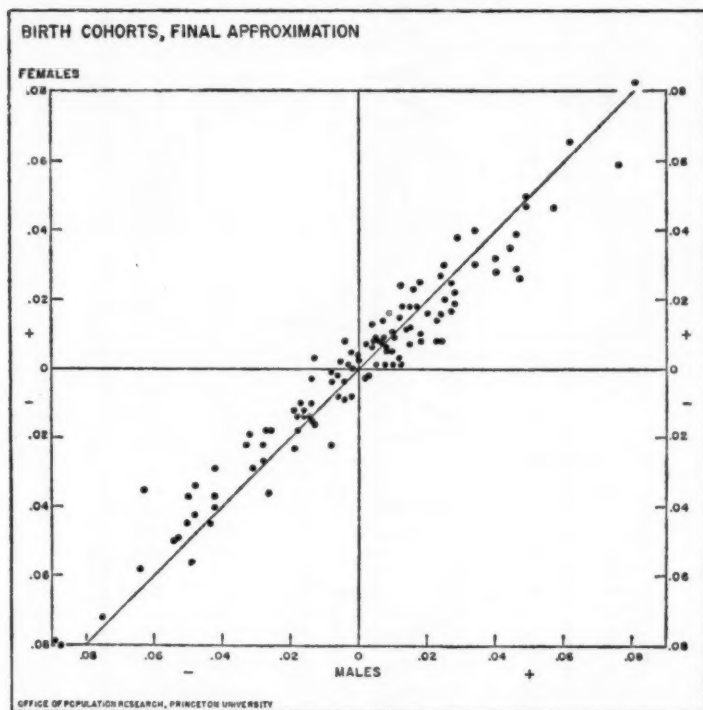


Fig. 5. Scattergram of average relative size of birth cohorts for white males and females, final approximation, 1820-1834.

two points: 1) as mentioned, the close correlation between the male and female series; 2) the large dips and rises in the births at periods of time when they would be "expected" (e.g., the hollow during the Civil War and the small size of the 1930 cohorts).<sup>25</sup>

These estimates of birth cohorts, while providing some confirmation of the method used and some indication of the relative size of each cohort, cannot be used to estimate the absolute size of the yearly birth cohorts. They are "relative figures" and therefore are affected by their neighbors; a cohort "bounded"

<sup>25</sup> The series actually extended back to the birth cohort of April 1805-March 1806. The earliest years are not shown simply to reduce the size of the figure. The correlation for the years eliminated is hardly lower than for all the years shown.

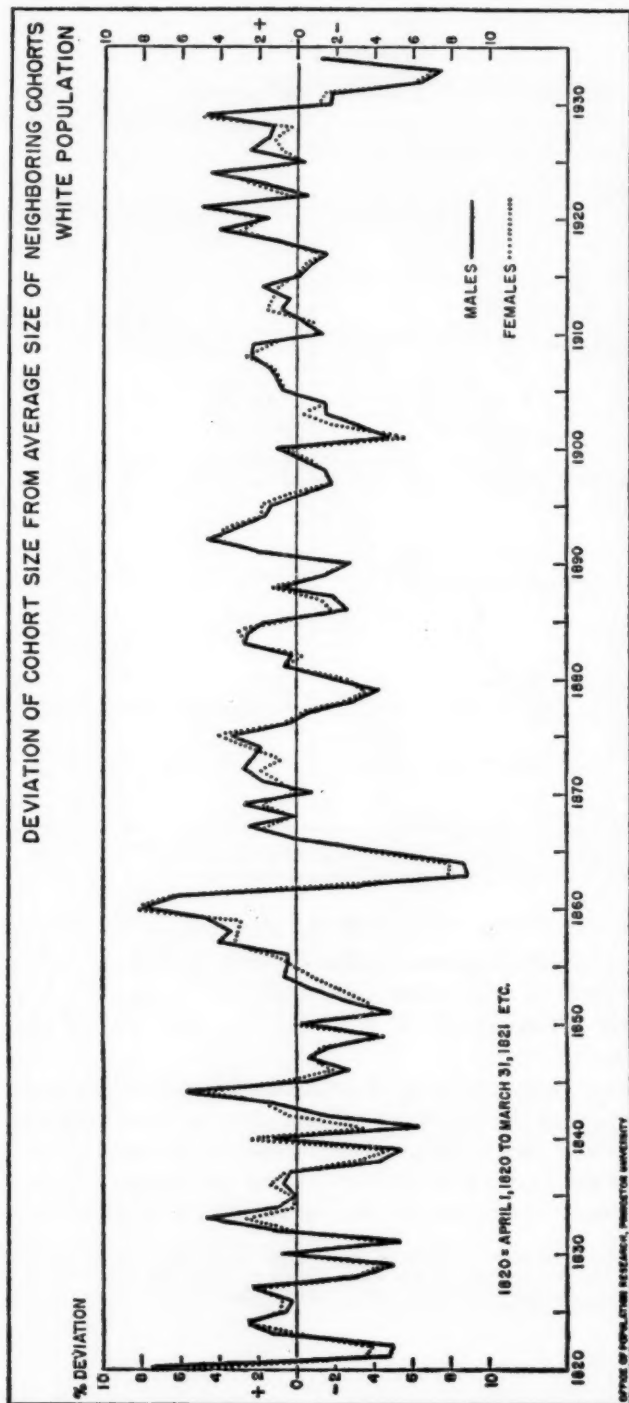


Fig. 6. Percentage deviation of white male and female birth cohorts from average size of ten neighboring birth cohorts, 1820-1934.

# Age Heaping in the 1880-1950 Census

555

Table 2. Age heaping adjustment factors, native white females, ages 5-85, 1880-1950.\*

Age	Census							
	1880	1890	1900	1910	1920	1930	1940	1950
5	1.027	1.025	1.001	1.022	1.020	1.018	1.016	1.014
6	1.055	1.046	1.007	1.028	1.019	1.010	1.001	.992
7	.980	.985	.984	.994	.998	1.003	1.007	1.012
8	1.021	1.019	1.010	1.015	1.014	1.012	1.010	1.008
9	.935	.946	.998	.968	.978	.989	1.000	1.011
10	1.032	1.024	1.044	1.008	1.000	1.000 <sup>a</sup>	1.000 <sup>a</sup>	1.000 <sup>a</sup>
11	.931	.939	.965	.955	.963	.971	.979	.987
12	1.062	1.056	.998	1.043	1.037	1.031	1.024	1.018
13	.960	.966	.984	.977	.982	.987	.993	.998
14	1.007	1.007	.988	1.007	1.008	1.008	1.008	1.008
15	.932	.940	.972	.954	.961	.968	.976	.983
16	1.033	1.030	1.004	1.022	1.019	1.015	1.012	1.008
17	.946	.954	.991	.970	.978	.986	.995	1.003
18	1.097	1.086	1.015	1.062	1.050	1.039	1.027	1.015
19	.949	.957	.986	.973	.981	.989	.997	1.005
20	1.090	1.070	1.050	1.031	1.011	1.000 <sup>a</sup>	1.000 <sup>a</sup>	1.000 <sup>a</sup>
21	.928	.940	.973	.963	.975	.986	.998	1.010
22	1.059	1.050	1.030	1.030	1.020	1.010	1.000	.990
23	1.011	1.009	1.009	1.005	1.004	1.002	1.000	.998
24	1.020	1.018	1.016	1.014	1.013	1.011	1.009	1.007
25	1.046	1.042	1.031	1.035	1.032	1.028	1.024	1.021
26	.986	.987	.973	.991	.993	.995	.996	.998
27	.920	.929	.970	.947	.956	.965	.974	.983
28	1.086	1.076	1.004	1.054	1.043	1.032	1.022	1.011
29	.828	.856	.941	.914	.941	.968	.995	1.022
30	1.295	1.258	1.127	1.184	1.146	1.107	1.068	1.030
31	.747	.774	.879	.827	.853	.879	.907	.932
32	1.009	1.014	.995	1.022	1.027	1.032	1.036	1.040
33	.918	.925	.962	.940	.947	.954	.961	.968
34	.907	.920	.972	.945	.958	.970	.983	.996
35	1.139	1.124	1.052	1.095	1.081	1.067	1.052	1.038
36	.985	.985	.976	.985	.984	.984	.984	.984
37	.895	.903	.955	.919	.927	.935	.943	.951
38	1.105	1.099	1.048	1.086	1.080	1.074	1.068	1.061
39	.891	.914	.992	.955	.976	.996	1.017	1.038
40	1.370	1.331	1.152	1.250	1.212	1.169	1.127	1.085
41	.752	.769	.867	.803	.820	.837	.854	.871
42	1.012	1.023	.998	1.047	1.058	1.070	1.082	1.094
43	.896	.906	.948	.928	.939	.950	.960	.971
44	.923	.923	.970	.923	.923	.923	.923	.923
45	1.207	1.181	1.069	1.129	1.103	1.076	1.050	1.024
46	.901	.906	.935	.914	.919	.924	.928	.932
47	.879	.892	.969	.917	.929	.941	.952	.962
48	1.060	1.058	1.027	1.052	1.050	1.047	1.044	1.041
49	.883	.907	.980	.950	.972	.994	1.015	1.037
50	1.436	1.395	1.217	1.310	1.267	1.227	1.182	1.137
51	.769	.785	.908	.815	.830	.845	.860	.875
52	1.024	1.029	1.000	1.038	1.042	1.047	1.051	1.056
53	.904	.913	.942	.931	.940	.949	.958	.967
54	.997	.997	.978	.997	.996	.996	.996	.996

Table 2 (Continued)

AGE	CENSUS							
	1880	1890	1900	1910	1920	1930	1940	1950
55	1.056	1.045	1.037	1.023	1.013	1.002	.991	.980
56	1.014	1.004	.982	.984	.974	.964	.954	.944
57	.852	.865	.935	.892	.906	.919	.932	.944
58	.994	.999	.978	1.010	1.015	1.021	1.026	1.032
59	.819	.848	.950	.907	.935	.963	.991	1.019
60	1.416	1.372	1.208	1.282	1.236	1.192	1.144	1.096
61	.742	.758	.863	.790	.806	.822	.838	.854
62	.996	.994	.978	.990	.988	.986	.985	.983
63	.989	.990	.999	.992	.993	.994	.995	.996
64	.961	.962	.983	.964	.965	.966	.967	.968
65	1.194	1.186	1.057	1.170	1.162	1.154	1.146	1.138
66	.917	.915	.960	.912	.910	.908	.906	.905
67	.890	.901	.966	.919	.928	.937	.946	.955
68	1.021	1.017	.976	1.010	1.006	1.003	.999	.996
69	.833	.858	.942	.910	.934	.959	.983	1.007
70	1.267	1.240	1.133	1.188	1.160	1.132	1.104	1.076
71	.722	.744	.860	.787	.809	.831	.852	.874
72	.996	.998	.989	1.004	1.006	1.009	1.012	1.014
73	.940	.944	.941	.953	.958	.962	.967	.971
74	.960	.959	.968	.957	.956	.955	.954	.953
75	1.094	1.090	1.055	1.081	1.076	1.072	1.068	1.063
76	.978	.974	.946	.965	.960	.956	.951	.947
77	.877	.881	.965	.891	.897	.902	.906	.911
78	1.023	1.014	.975	.994	.984	.974	.964	.954
79	.819	.842	.895	.889	.914	.936	.959	.981
80	1.166	1.148	1.054	1.114	1.097	1.080	1.063	1.046
81	.760	.778	.885	.814	.832	.850	.867	.885
82	.955	.959	.946	.966	.969	.973	.977	.980
83	.955	.960	.946	.971	.977	.982	.987	.993
84	1.082	1.068	.986	1.039	1.024	1.010	.996	.981
85	.982	.977	.942	.966	.960	.955	.950	.944

\* These figures were used as divisors to correct the census enumerated populations for age heaping. The figures for 1880, 1890, and 1910-1950 represent the adjusted second approximation trend lines, the derivation of which is described in the text and Appendix B. The figures for 1900 are the life table adjusted age ratios corrected for cohort size.

<sup>1</sup> These age ratios have not been allowed to go below unity by assumption; see pp. 558 and 563.

by smaller sized cohorts appears larger than it actually should.

The second approximation trend line values, or the age heaping adjustment factors, were used to correct the enumerated populations. In the case of 1900, the original age ratios as corrected by the final approximation of cohort size were used to adjust the enumerated population.<sup>26</sup> This procedure was followed because of the afore-mentioned deviation of 1900 from

<sup>26</sup> Technically speaking, the adjustment factors are the second approximation trend line values corrected by the adjustment described in Appendix B.



Table 3. Age heaping adjustment factors, native white males, ages 5-85, 1880-1950.\*

Age	Census							
	1880	1890	1900	1910	1920	1930	1940	1950
5	1.021	1.021	1.003	1.021	1.020	1.020	1.020	1.020
6	1.041	1.035	1.010	1.022	1.016	1.010	1.004	.997
7	.977	.981	.980	.990	.995	.999	1.004	1.008
8	1.009	1.009	1.003	1.009	1.008	1.008	1.008	1.008
9	.937	.949	1.003	.972	.984	.996	1.007	1.019
10	1.053	1.040	1.049	1.015	1.003	1.000 <sup>a</sup>	1.000 <sup>a</sup>	1.000 <sup>a</sup>
11	.932	.938	.965	.950	.957	.963	.969	.976
12	1.080	1.073	1.007	1.058	1.051	1.044	1.037	1.030
13	.968	.972	.994	.979	.983	.986	.990	.994
14	1.040	1.036	.997	1.029	1.025	1.022	1.018	1.014
15	.932	.940	.975	.954	.961	.968	.976	.983
16	1.004	1.007	.999	1.013	1.015	1.018	1.021	1.023
17	.948	.959	.988	.980	.991	1.002	1.013	1.023
18	1.022	1.022	.991	1.022	1.022	1.022	1.022	1.022
19	.970	.975	.970	.984	.988	.993	.997	1.002
20	.962	.960	1.013	.954	.951	.949	.946	.943
21	1.058	1.050	1.032	1.036	1.029	1.022	1.014	1.007
22	1.034	1.027	1.025	1.013	1.005	.998	.991	.984
23	1.037	1.029	1.004	1.013	1.004	.996	.988	.980
24	1.011	1.010	1.010	1.008	1.007	1.006	1.005	1.004
25	1.000	.999	1.011	.997	.996	.996	.995	.994
26	.978	.980	.964	.986	.988	.991	.994	.996
27	.958	.964	.975	.977	.983	.989	.996	1.002
28	1.094	1.081	1.013	1.056	1.043	1.031	1.018	1.005
29	.873	.899	.958	.946	.969	.993	1.016	1.040
30	1.296	1.253	1.144	1.165	1.119	1.073	1.027	.981
31	.779	.803	.877	.850	.873	.898	.921	.943
32	.985	.990	.986	1.001	1.006	1.012	1.017	1.022
33	.927	.934	.960	.949	.956	.963	.970	.978
34	.910	.926	.968	.959	.975	.991	1.007	1.023
35	1.161	1.142	1.067	1.104	1.086	1.067	1.048	1.029
36	.948	.954	.968	.967	.973	.979	.986	.992
37	.880	.897	.954	.926	.941	.955	.969	.984
38	1.097	1.088	1.057	1.070	1.061	1.052	1.043	1.034
39	.928	.945	.994	.979	.996	1.014	1.031	1.048
40	1.345	1.303	1.160	1.221	1.177	1.133	1.089	1.045
41	.791	.807	.874	.839	.855	.871	.887	.905
42	1.003	1.015	.991	1.040	1.053	1.066	1.078	1.091
43	.878	.895	.929	.924	.938	.952	.967	.981
44	.887	.892	.960	.903	.907	.912	.916	.921
45	1.232	1.202	1.098	1.140	1.110	1.079	1.049	1.018
46	.907	.915	.930	.932	.940	.948	.956	.964
47	.881	.899	.958	.930	.945	.960	.976	.991
48	1.023	1.022	1.010	1.018	1.016	1.014	1.013	1.011
49	.899	.921	.976	.964	.986	1.007	1.029	1.050
50	1.372	1.332	1.216	1.252	1.213	1.171	1.129	1.086
51	.817	.828	.937	.851	.862	.873	.884	.897
52	1.076	1.075	1.042	1.073	1.072	1.071	1.070	1.069
53	.959	.962	.971	.969	.973	.977	.980	.984
54	.968	.973	.970	.982	.986	.991	.996	1.000

Table 3 (Continued)

Age	Census							
	1880	1890	1900	1910	1920	1930	1940	1950
55	1.036	1.025	1.017	1.004	.993	.982	.971	.960
56	1.001	.996	.965	.985	.979	.974	.968	.963
57	.879	.889	.925	.909	.918	.927	.936	.945
58	.968	.972	.968	.981	.986	.990	.995	.999
59	.820	.851	.944	.912	.941	.969	.998	1.027
60	1.320	1.284	1.170	1.212	1.173	1.134	1.095	1.057
61	.787	.799	.879	.821	.833	.844	.855	.866
62	1.030	1.025	.999	1.016	1.012	1.007	1.003	.998
63	1.028	1.026	1.021	1.022	1.021	1.019	1.017	1.015
64	.950	.956	.985	.967	.972	.978	.983	.988
65	1.149	1.143	1.028	1.130	1.124	1.118	1.112	1.105
66	.919	.915	.951	.908	.905	.901	.897	.894
67	.895	.906	.992	.930	.942	.953	.965	.977
68	1.022	1.014	.979	.996	.986	.978	.968	.960
69	.878	.899	.942	.937	.956	.975	.994	1.013
70	1.199	1.174	1.096	1.123	1.098	1.073	1.048	1.022
71	.782	.800	.891	.836	.853	.871	.889	.908
72	1.014	1.017	1.001	1.022	1.025	1.028	1.031	1.033
73	.958	.962	.977	.971	.976	.980	.985	.989
74	.963	.966	.971	.971	.974	.977	.979	.982
75	1.032	1.032	1.018	1.030	1.029	1.028	1.027	1.026
76	.962	.960	.934	.954	.951	.949	.946	.943
77	.864	.873	.987	.892	.903	.912	.921	.930
78	1.041	1.029	1.003	1.004	.991	.978	.966	.953
79	.858	.879	.911	.924	.944	.965	.986	1.006
80	1.142	1.120	1.008	1.075	1.052	1.030	1.007	.985
81	.808	.822	.914	.851	.865	.879	.895	.908
82	.975	.974	.959	.972	.971	.970	.969	.968
83	.943	.951	.941	.968	.976	.984	.992	1.000
84	1.023	1.021	.977	1.015	1.013	1.010	1.007	1.004
85	.926	.925	.920	.924	.923	.922	.921	.920

\* These figures were used as divisors to correct the census enumerated populations for age heaping. The figures for 1880, 1890, and 1910-1950 represent the adjusted second approximation trend lines, the derivation of which is described in the text and Appendix B. The figures for 1900 are the life table adjusted age ratios corrected for cohort size.

† These age ratios have not been allowed to go below unity by assumption; see pp. 558 and 563.

the trend in age heaping. These adjustment factors, for both sexes, are shown in Tables 2-3.

Figures 7-14 show the enumerated and adjusted native white female populations for each of the eight censuses from 1880 to 1950. The adjusted populations, while showing much smoother age distributions, retain genuine differences in cohort size.

The age heaping adjustment factors just described resulted in an irregular pattern in the adjusted 1930, 1940, and 1950

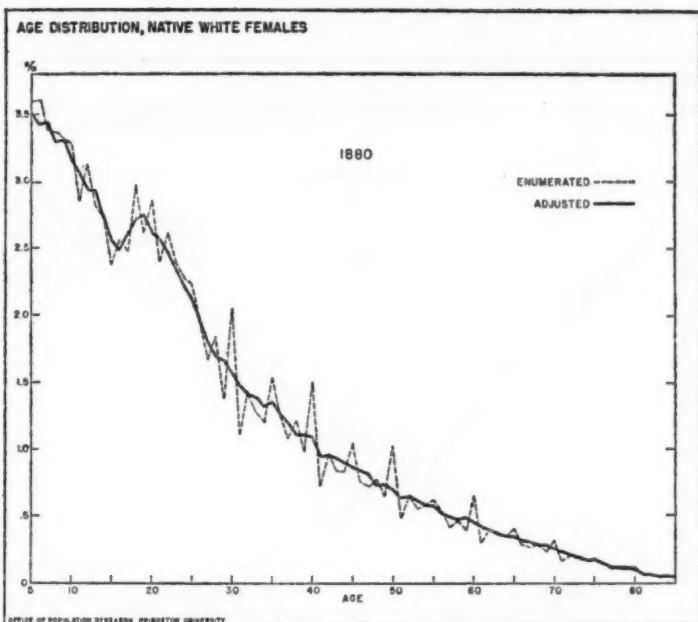


Fig. 7. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1880. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

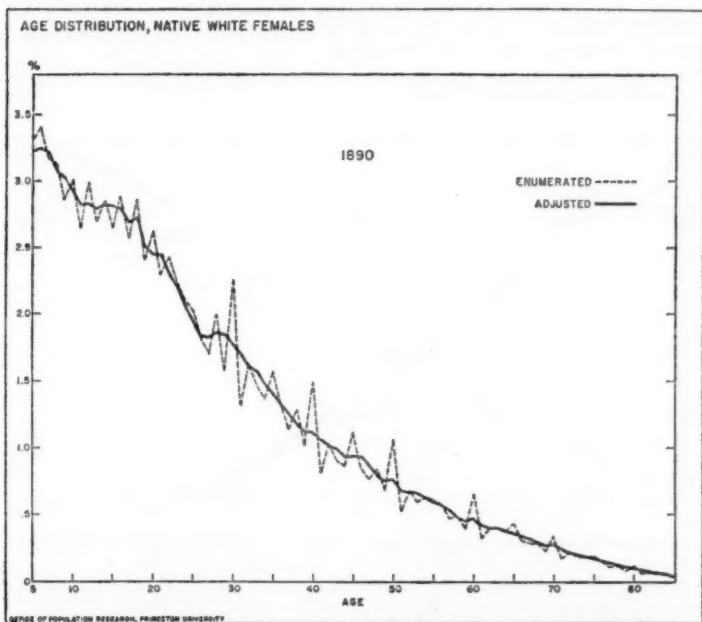


Fig. 8. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1890. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

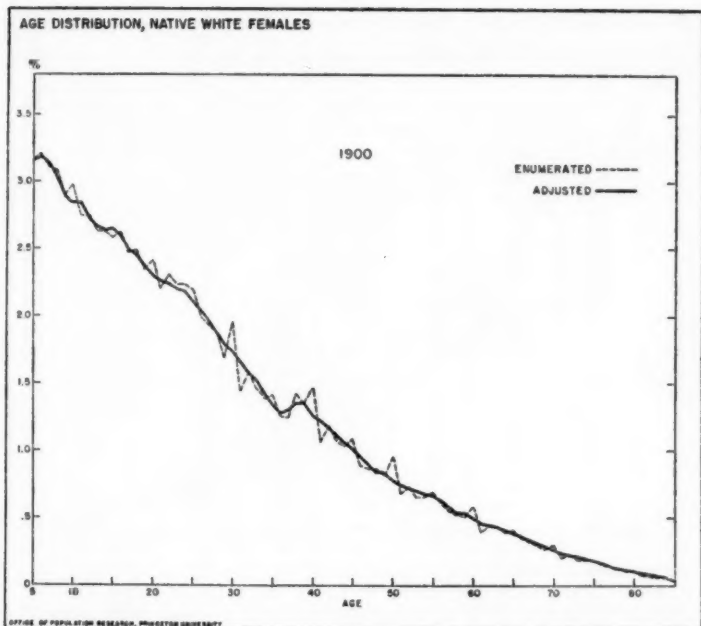


Fig. 9. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1900. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.



Fig. 10. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1910. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

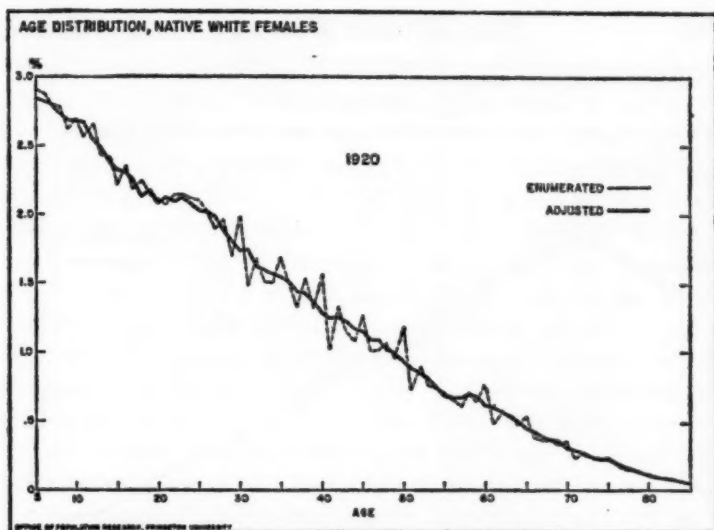


Fig. 11. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1920. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

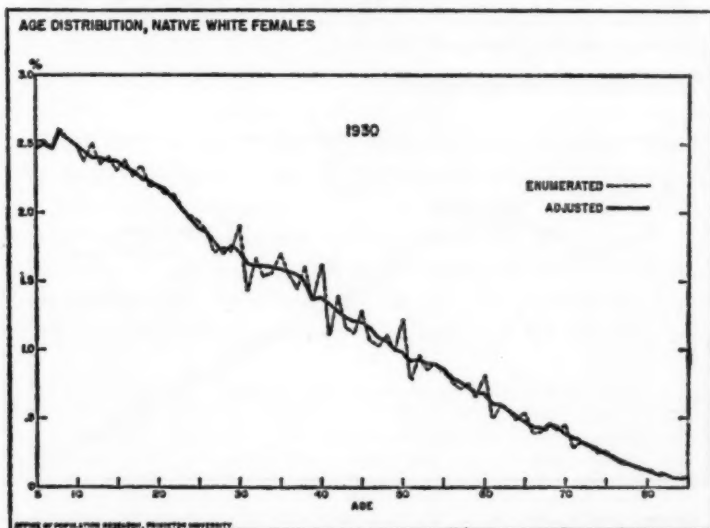


Fig. 12. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1930. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

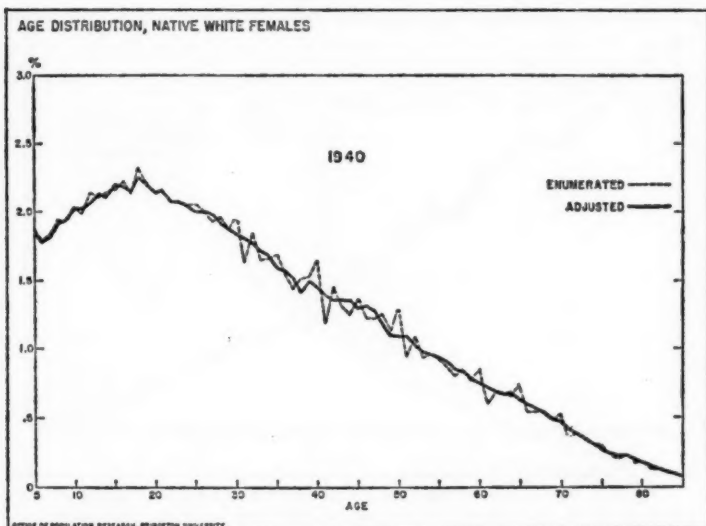


Fig. 13. Percentage distribution of native white females, ages 5-85 as enumerated by the census and as adjusted for age heaping: 1940. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

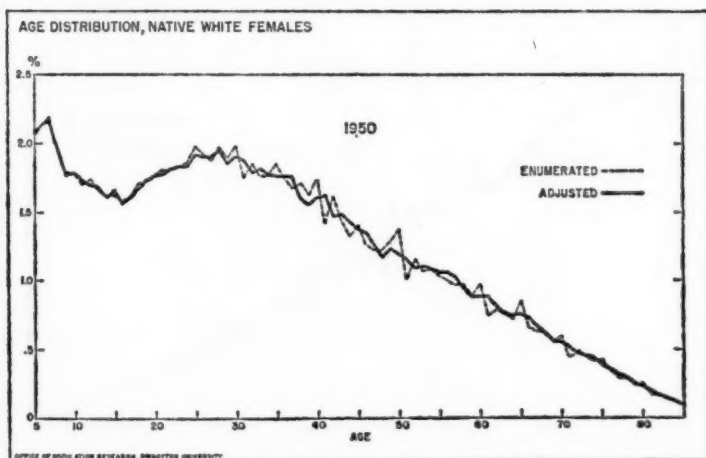


Fig. 14. Percentage distribution of native white females, ages 5-85, as enumerated by the census and as adjusted for age heaping: 1950. Sources: Adjusted population derived by use of correction factors shown in Table 2; census enumerated population, footnote 3.

values at ages 10 and 20. This irregularity seemed to be traceable to the original assumption of a linear trend in age heaping. It should be obvious that while a linear trend may be valid for a series of censuses, it cannot continue indefinitely through time, for if such were the case all preferred ages would eventually become avoided and vice versa. In other words, if age heaping for age 40, for example, has diminished in a linear trend, a continuation of this trend would lead to age 40 becoming increasingly avoided, a conclusion which is highly doubtful. It appears that the assumption of linearity in age heaping led to the improbable result of avoidance of young ages ending in 0 in 1930, 1940, and 1950. What is probably more accurate for these ages is a leveling off of the age heaping rather than a continuation of a linear trend which leads to avoidance. To the degree that this conjecture is correct, the assumption of linearity resulted in an improper correction being applied to the enumerated populations at these ages.

On the basis of this reasoning,<sup>27</sup> the assumption was made that the age heaping adjustment factors for ages 10 and 20 in 1930, 1940, and 1950 should not be allowed to go below unity. A correction factor of 1.000, therefore, was used to adjust the enumerated populations at these ages. The same assumption was applied to the males but only for age 10 since the age heaping trend line for age 20 was continuously below unity.<sup>28</sup> The adjusted populations resulting from the use of this correction factor are included in Figures 12-14.

In Table 4, the per cent of age heaping for each sex in 1950 is shown for each terminal digit of age. In general, those ages which are preferred by males are preferred by females, while

<sup>27</sup> Further evidence confirming this reasoning resulted from the birth estimates which were subsequently generated from the age heaping adjusted populations.

<sup>28</sup> If the trend of age heaping for ages 10 and 20 in 1930, 1940, and 1950 has been "leveling off" and approaching unity asymptotically, rather than linearly, then the "true" correction factor should be slightly higher than 1.000. The use of 1.000, however, prevents an over correction of the enumerated populations and serves as a useful approximation of what the lower limit of the age heaping correction factor should have been. The difference between the "true" correction factor and the modified value used, 1.000, is probably negligible.



Table 4. Per cent of over and under age-preference of native white males and native white females, ages 5-85, 1950.

AGE	MALE	FEMALE	AGE	MALE	FEMALE
TERMINAL DIGIT 5			TERMINAL DIGIT 6		
5	2.0	1.4	6	-0.3	-0.8
15	-1.7	-1.7	16	2.3	0.8
25	-0.6	2.1	26	-0.4	-0.2
35	2.9	3.8	36	-0.8	-1.6
45	1.8	2.4	46	-3.6	-6.8
55	-4.0	-2.0	56	-3.7	-5.6
65	10.5	13.8	66	-10.6	-9.5
75	2.6	6.3	76	-5.7	-5.3
85	-8.0	5.6			
TERMINAL DIGIT 7			TERMINAL DIGIT 8		
7	0.8	1.2	8	0.8	0.8
17	2.3	0.3	18	2.2	1.5
27	0.2	-1.7	28	0.5	1.1
37	-1.6	-4.9	38	3.4	6.1
47	-0.9	-3.8	48	1.1	4.1
57	-5.5	-5.6	58	-0.1	3.2
67	-2.3	-4.5	68	-4.0	-0.4
77	-7.0	-8.9	78	-4.7	-4.6
TERMINAL DIGIT 9			TERMINAL DIGIT 0		
9	1.9	1.1	10	0.0	0.0
19	0.2	0.5	20	-5.7	0.0
29	4.0	2.2	30	-1.9	3.0
39	4.8	3.8	40	4.5	8.5
49	5.0	3.7	50	8.6	13.7
59	2.7	1.9	60	5.7	9.6
69	1.3	0.7	70	2.2	7.6
79	0.6	-1.9	80	-1.5	4.6
TERMINAL DIGIT 1			TERMINAL DIGIT 2		
11	-2.4	-1.3	12	3.0	1.8
21	0.7	1.0	22	-1.4	-1.0
31	-5.7	-6.8	32	2.2	4.0
41	-9.5	-12.9	42	9.1	9.4
51	-10.3	-12.5	52	6.9	5.6
61	-13.4	-14.6	62	-0.2	-1.7
71	-9.2	-12.6	72	3.3	1.4
81	-9.2	-11.5	82	-3.2	-2.0
TERMINAL DIGIT 3			TERMINAL DIGIT 4		
13	-0.6	-0.2	14	1.4	0.8
23	-2.0	-0.2	24	0.4	0.7
33	-2.2	-3.2	34	2.3	-0.4
43	-1.9	-2.9	44	-7.9	-7.7
53	-1.6	-3.3	54	0.0	-0.4
63	1.5	-0.4	64	-1.2	-3.2
73	-1.1	-2.9	74	-1.8	-4.7
83	0.0	-0.7	84	-8.0	-1.9

Sources: Table 2 for the females and Table 3 for the males.

those ages avoided by males are also avoided by females. In most cases, however, the degree of avoidance or preference is higher for females than for males.

Table 4 also points up the possible dangers involved in discussing digital preferences, thereby obscuring the range of preference and avoidance for specific ages rather than digits. It has been common to discuss age heaping as being over in ages ending in 5, 0, and even numbers, with ages ending in odd numbers being avoided. Table 4 shows this to be an over-generalization. For most digits there is heaping in both directions. Ages ending in 4 and 6 are generally avoided, probably the result of being adjacent to ages ending in 5. At the same time, some of the ages ending in odd numbers (other than 5) are preferred.

## APPENDIX A

### CORRECTION OF THE 1930, 1940, AND 1950 CENSUSES

In the 1930 census, Mexicans were not included as white, as they previously and subsequently were, but were classified as nonwhite, under "other races." There was a total of 409,672 native male Mexicans and 395,482 native female Mexicans, shown only by a five year age distribution. (These figures do not include those reported at "age unknown.")<sup>1</sup> Single year estimates were arrived at by redistributing these numbers in the same proportion within each five year age group as the native white of foreign or mixed parentage. These figures were then added to the figures given for native whites, the sum representing a total native white population comparable to other censuses.

Single year age distributions for the native white male and native white female populations are given in the 1940 census only for 35 years of age and over; under 35 years the classification is by five year age groups. There is, however, a single year age distribution, by sex, for the total white population.<sup>2</sup> The numbers of foreign white up to age 29 were relatively small; only in the 30-34 years of age group did they amount to a significant proportion (7.5 per cent for the males

<sup>1</sup> U.S. Bureau of the Census, *1930 Census of Population*, op. cit., Table 15, p. 586.

<sup>2</sup> U.S. Bureau of the Census, *1940 Census of Population*, op. cit., Table 2, p. 9.

and 7.9 per cent for the females). It therefore seemed justifiable to redistribute the 0-35 native white population to single years by the per cent distribution of total whites within each five year age group.

In the 1950 census, a total of 337,290 native white males were reported abroad in 1950.<sup>3</sup> Although this represents only 0.5 per cent of the total native white male population, there is a fairly high degree of concentration in the 20-34 years of age group. It therefore seemed advisable to make some adjustment to the figures shown for the native white males, which are only for continental United States.<sup>4</sup> No adjustment was made for the native white females since the number abroad represented only 0.1 per cent of the total, with a much smoother distribution over the entire age range.

The native white males abroad in the age ranges 0-14, and 40 and over, were allocated in the same proportions as the continental native white males. For ages 15-39, a single year age distribution was arrived at by using the same percentage age distribution of white males in the armed forces stationed in continental United States. This latter figure was determined by subtracting the civilian labor force from the total labor force, the difference representing the males in the armed forces.<sup>5</sup>

## APPENDIX B

### CALCULATION OF TRUE CORRECTION FACTOR FOR AGE HEAPING

It can easily be demonstrated that if a series of numbers is linear, the ratio of the mid-point of the series divided by the average of the ten adjacent numbers will be unity. If the assumption is made that the correct age distribution is a linear trend, then  $\frac{n+r}{n}$  would be the true index of age heaping, where  $n$  represents the average of the number at the ten ages adjacent to any age, and  $r$  the net number of persons misreporting themselves at that age. This however ignores the fact that the  $r$  persons added to the numerator are "drawn out" of other ages. If a preferred age "draws" on people from within the

<sup>3</sup> U.S. Bureau of the Census, *1950 Census of Population, op cit.*, Table 35, p. 1-87.

<sup>4</sup> *Ibid.*, Table 94, pp. 1-165.

<sup>5</sup> U.S. Bureau of the Census: *Employment and Personal Characteristics, Part 1, Chapter A in U.S. Bureau of Population: 1950, Vol. IV (Special Reports)*, Table 1, p. 1A-22.

span used to determine the denominator, then the effect is to decrease the denominator by .10r; i.e., the actual ratio is  $\frac{n+r}{n-\frac{r}{10}}$ .

If it is assumed that persons incorrectly reporting a given age typically have an age within five years of the one reported, the above argument shows that an "age ratio" is not an appropriate correction factor to remove (by division) the effect of age heaping. It can be shown that  $CF = \frac{11AR}{10+AR}$  where AR represents the age ratio and CF the true correction factor. If  $AR = \frac{n+r}{n-\frac{r}{10}}$  and  $CF = \frac{n+r}{n}$

$$\text{then } AR = \frac{n+r}{n} \times \frac{1}{1-\frac{r}{10n}}$$

$$AR = CF \times \frac{1}{1-\frac{r}{10n}}$$

$$CF = AR \left(1 - \frac{r}{10n}\right)$$

$$CF = AR \left(1 - \frac{CF}{10} + \frac{1}{10}\right)$$

$$CF = AR \left(\frac{11}{10} - \frac{CF}{10}\right)$$

$$CF = AR \left(\frac{11}{10}\right) - AR \left(\frac{CF}{10}\right)$$

$$CF + AR \left(\frac{CF}{10}\right) = AR \left(\frac{11}{10}\right)$$

$$CF \left(1 + \frac{AR}{10}\right) = AR \left(\frac{11}{10}\right)$$

$$CF = \frac{\frac{11}{10} AR}{1 + \frac{AR}{10}}$$

$$CF = \frac{11AR}{10+AR}$$

This adjustment was applied to the age heaping correction factors

(i.e., the second approximation trend line values) and is incorporated in the figures shown in Tables 2 and 3.

## APPENDIX C

### ADJUSTMENT OF AGE RATIOS FOR CERTAIN AGES

Age ratios for ages ending in 5 were calculated with a denominator containing two ages ending in 0. Since ages ending in 0 appear to be heavily preferred ages, at least for age 30 and on, this would tend to depress the age ratios for those ages ending in 5. The same type of factor concerns the age ratios for those ages ending in 4 and 6 (where the former has a double 9 in the denominator and the latter a double 1—in both cases a digit which is generally thought to be avoided)<sup>6</sup> except that these ratios would be inflated. Since the over preference for 0 seems to be largely at the expense of the two adjacent ages (those ending in 9 and 1), the average of the three would tend to approximate the "true" figure for each age.

On the basis of this reasoning, age ratios for ages ending in 4, 5 and 6 were calculated slightly differently from those of other ages. The ages ending in 9, 0 and 1 were averaged with this average figure being included in the denominator the appropriate number of times, i.e., instead of the original numbers representing the 0 and 1 ages (in the case of an age ratio for an age ending in 5), twice the average of the ages ending in 9, 0 and 1 was used. This correction is of negligible proportions and is included in the figures shown in Table 1. Because of the small size of this correction, however, it did not seem warranted to correct the age ratios for ages ending in 0, which have a double 5 in the denominator (a digit which is less preferred than 0), or for ages ending in 1, 2, 3, 7, 8 and 9.

## APPENDIX D

### USE OF BIRTH REGISTRATION DATA IN ELIMINATING COHORT SIZE FROM AGE RATIOS

There is a long history of birth registration for a small number of states. An attempt was made to use these on the provisional assump-

<sup>6</sup> However, see Table 4 where, in 1950, ages ending in 9 are shown as preferred rather than avoided.

tion that they were a representative sample of fluctuations in the fertility behavior of the entire country. The states used and the periods covered were Connecticut and Massachusetts, 1853-1888; Connecticut, Massachusetts and New Hampshire, 1889-1896; Connecticut, Massachusetts, New Hampshire and Maine, 1897-1919; the ten original states of the Birth Registration area, 1920-1937; from 1933 on, birth statistics for the total United States. As these births were registered by calendar year, it was first necessary to convert them to census-year births, that is, to June 1-May 31 births and to April 1-March 31 births. These census-year births were then transformed into birth ratios by the same method used to calculate the age ratios—the ratio of one year to the average of its ten adjacent neighbors. The birth ratios were then used in an effort to eliminate the cohort effect from the age ratios.

It should be clear from the method of calculating the ratios that it is not the absolute numbers which are of importance but the relative numbers. A proportional increase in the numbers for all ages, or births for all years, will in no way affect the ratios. Furthermore, where a series of births (regardless of what per cent it may be of the total absolute figures) is distributed in the same proportions as the total figures, the birth ratios are the same.

At the same time it should be obvious that the sudden "inclusion" of more states (in calculating the birth ratios) would result in meaningless figures. The "splicing" was therefore accomplished by treating each series of states separately until 5 years had elapsed. Thus, New Hampshire birth statistics, first available in 1884, meant that birth ratios up to 1888 were based on Connecticut and Massachusetts figures only. The inclusion of New Hampshire in 1884 allowed for calculation of the 1889 birth ratio using Connecticut, Massachusetts and New Hampshire data. This procedure was followed throughout.

Certain difficulties inhered in the use of this "spliced" series of birth ratios. First, it is extremely doubtful that the fertility behavior of the states used was actually representative of the fertility behavior of the total population of the United States. Secondly, the degree of under registration in Massachusetts ranged from 18 per cent in 1850 to 3.3 per cent in 1890.<sup>7</sup> Although there is no evidence, it is highly probable that the birth registration completeness changed in a

<sup>7</sup> Gutman, Robert: *The Birth Statistics of Massachusetts During the Nineteenth Century. Population Studies*, July 1956, Vol. X, No. 1: Table 2, p. 76.

similar fashion for the other states. It is exactly this changing rate of completeness which, while not influencing the number of people reported in a census, would adversely affect the birth ratios. On the basis of these considerations, this method of adjusting the age ratios for cohort size was dropped for the method described in the text.

## APPENDIX E

### EFFECT OF DIFFERENCES IN CENSUS DATE AND QUESTION ON AGE

The 1950, 1940, and 1930 censuses were taken as of April 1 of the censal year. The 1910 census was taken as of April 15 but has been treated in this paper as if it had been taken on April 1, the difference of 14 days being considered insignificant. The 1920 census was effective as of January 1, while the date of the 1900 and 1880 censuses was June 1. For all of these censuses, the question pertaining to age was "age last birthday" (and in 1900 the additional question of "date of birth," as previously mentioned). The differences in census date mean that we are not always dealing with the same birth cohort separated by ten year intervals. Thus, those reporting themselves at age  $x + 10$  in the 1910 census are not the survivors of the group of people reporting themselves at age  $x$  in the 1900 census. The effect of the question "age last birthday" is to make the mean age of persons reporting themselves at any age  $x + .5$  rather than  $x$ .

The 1890 census was taken as of June 1. The age question for this census however was "age at nearest birthday" instead of the more usual one "age at last birthday." This different question had the effect (if accurately answered) of centering the mean age of persons reporting at any age at  $x$  and not  $x + .5$  (in addition to "moving up" the reporting age by one year). The birth cohort born in 1885 was reported as age 5 in the 1890 census but the same (or approximately the same) birth cohort was reported as of age 14 in 1900.

The differences in the questions pertaining to age and in the census dates, and thereby reflecting different birth cohorts in advancing from one census to the next, necessitated rough approximations to arrive at the same birth cohort. In determining the age heaping adjustment factors, and the relative size of the birth cohorts (see Figure 6), an attempt was made to make all measurements as of April 1,



the most common of the census dates. For example, of the cohort born April 1, 1874-March 31, 1875, 5/6 were age 5 in 1880 and 1/6 age 6; in 1890 1/3 were age 15 while 2/3 were age 16; in 1900 5/6 were age 25 and 1/6 age 26; all were age 34 in 1910; in 1920 1/4 were age 43 and 3/4 age 44; in 1930 all were age 54, in 1940 age 64, and in 1950 age 74. In measuring the deviation of an age ratio from its various trend lines it was not possible to take account of these differences.

It was however possible to allow for the differences in correcting the age ratios for the effect of cohort size. This was done by reallocating the cohorts by the proper weights—those already given. This method may have affected the precision of the estimate of relative cohort size; it does not however diminish in any sense the correlation between the series for males and females since identical procedures were used for both sexes. Further, since the trend line of age heaping is continuous, it represents the degree of age heaping at each point in time.

## APPENDIX F

### ESTIMATION OF THE EFFECT OF COHORT SIZE ON THE TREND LINES

If the cohort size is known to be  $d$  then one end of each trend line is raised by  $\frac{d}{n_1}$  where  $n_1$  represents the number of age ratios used in establishing that end of the trend line.

Let  $n_1$  = number of age ratios determining position of one end of trend line

$N$  = number of times a cohort is observed

$d'$  = estimated size of cohort as determined by the average of the deviations from the several original trend lines

$$d = \text{true size of cohort} = d' + \frac{d}{n_1}$$

$$\text{then } \left( \frac{d}{n_1} \right) = \frac{\text{sum of effects when affected}}{\text{number of times observed}} = \text{the error in estimating the}$$

cohort size due to the effect of this cohort on each of its several trend lines. If this error is  $rd$ , then the estimate  $d' = (1-r)d$  or  $d = \frac{d'}{(1-r)}$ .

## APPENDIX G

THE EFFECT OF SOCIAL SECURITY LEGISLATION ON THE  
PATTERNS OF AGE HEAPING

There has been some speculation as to the effect of old age assistance legislation enacted in the 1930's on patterns of age heaping.<sup>8</sup> According to this view, ages 60-64 were relatively understated in 1940 and 1950, whereas ages 65-69 were heavily overstated with the former reporting themselves at the latter ages so as to be eligible to receive old age benefits.

A test of this "hypothesis" was made by treating ages 62-68 in a slightly different way (that is, in addition to the way in which all ages, including these, were treated). Since the change is supposed to have occurred in 1940 and 1950, age heaping for these ages in these two censuses would not follow the linear trend originally assumed to exist. The pattern would instead be curvilinear. For these ages therefore, the second end of each trend line was determined by averaging the values of 1910, 1920, and 1930 and centering it on 1920. Values after this date were determined by extrapolation.

The average relative size of each cohort was then determined in the same manner as explained in the text except that the deviations for ages 62-68 in 1940 and 1950 were not included in the estimation. In this respect these ages were treated in a fashion similar to the way in which 1900 was treated.

After adjusting the age ratios for cohort size and plotting the cohort adjusted values, certain features were revealed which threw some doubt on the supposed differences of these ages in 1940 and 1950. First, the "closeness of fit" between these values and their trend lines was not as good as had been the case when these ages had not been treated differently from the other ages.

Secondly, for some ages, the divergence from the linear trend was as large, or almost as large, in 1930 as in 1940 and 1950. Since the old age assistance legislation had not been enacted until the middle 1930's, it cannot explain this. Thirdly, for certain ages, notably 64 and 68 (for both sexes), the cohort adjusted age ratios went in a direction opposite to what one would expect—age 64 showed an increase in heaping while age 68 showed a decrease. This result also

<sup>8</sup> See e.g., Coale, Ansley J., *op. cit.*, p. 20, and U.S. Bureau of the Census, *1940 Census of Population*, *op. cit.*, p. 3.

tends to contradict the hypothesis of changing patterns resulting from the legislation.

It also appears that different estimates of age heaping could have been obtained for any age, on the basis of shortened trend lines. In view of these considerations, it was decided to use the results obtained originally and to ignore the supposed change in the age heaping of these ages in the 1940 and 1950 censuses. This test is not intended as a denial of the hypothesis that there was a change in the patterns of age heaping resulting from the legislation mentioned. It does however suggest that the simple cause and effect relationship posited is not obvious when dealing with single year age distributions rather than 5 year groups.



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